ESSAYS ON URBAN DEPOPULATION AND MUNICIPAL POLICY

By

Christina Plerhoples

A DISSERTATION

Submitted to
Michigan State University
in partial fulfillment of the requirements
for the degree of

Agricultural, Food and Resource Economics – Doctor of Philosophy

2013

ABSTRACT

ESSAYS ON URBAN DEPOPULATION AND MUNICIPAL POLICY

By

Christina Plerhoples

Much of the academic literature on urban economics and public finance focuses on growth – both population growth and budgetary growth. However, most cities do not grow continuously, and many cities face long periods of decline rather than expansion. The city of Detroit, for example, dropped in population from 1.8 million in 1950 to 713,777 in 2010. The city of Saginaw, which is examined in Chapter 1 of this dissertation, went from a peak population of 98,265 in the 1960s to 51,230 in 2013. It is difficult for city governments to maintain infrastructure and provide adequate public services to a city that has lost such a massive amount of population and with it, revenue streams. Little research exists on policies that can be used by such cities. In this dissertation, I analyze two such policies.

In the first essay, I examine the effect of vacant building demolitions on crime using block face level monthly panel data from the city of Saginaw, Michigan. Although the United States Government spends millions of dollars a year on vacant building demolitions, no clear causal link has been established between demolitions and crime. Results from my analysis indicate that both demolitions and permits for demolitions actually increase crime rather than reduce it. These results imply that by the time a building reaches the point of demolition, it may be too late to reverse the crime trends incurred by neighborhood decline.

In the second essay, I examine the motivations for why cities choose to implement public employee residency requirement laws, or laws that require employees to live within the city boundaries. Justifications for these laws range from public safety to public coffer arguments.

However, little is known about the true motivations for why cities choose to implement residency laws. In this paper, I estimate a parametric duration model with a proportional hazard function to examine these motivations. In order to perform this analysis, I construct a unique data set on municipal residency laws for the largest 100 cities in the United States from 1970 through 2007. I also create a set of fiscal stress indicators for each of these cities over time. Results indicate that cities implement residency laws for economic and racial equity reasons and remove them when the city improves in comparison to the metropolitan area as a whole. Fiscal stress and service provision quality do not appear to be major contributing factors.

In the third essay, I look once again municipal residency requirement laws and examine the impact that these laws have on cities. Despite the prevalence and extensive history of residency laws, little is known about their impact on city outcomes. In this paper, I provide the first causal estimates of the impact of municipal residency requirements on the quality of service provision, municipal fiscal health, and the size and composition of cities. My identification strategy exploits the timing of municipal law changes as well as the implementation of state bans on residency laws. Results provide little evidence that residency laws have an effect on fiscal health or city demographics when they are implemented, but they are correlated with a temporary increase then long term decrease in crime. The removal of a residency law is not associated with any significant change in fiscal health, city demographics, or crime.

These three essays provide information that can be used to help declining cities avoid spending time and money on policies that do not work and remove laws that are harmful. More research is needed to determine what policies do work in declining cities and how they can best be implemented.

DEDICATION

I dedicate this dissertation to my family and friends, whose never ending support made this process not only possible, but enjoyable.

To my mom and dad, the two smartest, kindest, and most generous people I know. You have always put yourselves second and have always been supportive, whether it was talking to me at midnight from the other side of the world, or traveling last minute to help me in a crisis. I love you!

To my brother Bill, who sometimes knew more about me than I did. You helped me choose the right path at many forks in the road.

To my brother Tim, who I have always looked up to and who has never let me down.

To my sister-in-law Ellie, whose grace and composure are something I strive to achieve.

To my sister-in-law Alicia, who is the epitome of strong female role model.

To Lyra, Cora, Matthew, and Naomi, who will hold my heart forever.

To Anna and Kelly and the power of three.

To Brian, who makes it all worth it. Sont des mots qui vont très bien ensemble, Très bien ensemble.

To Tim, Drea, Dale, Sung, Nicky, Laura, Rie, Susan, Xuan, Adam, and too many more to list.

In memory of Asa.

ACKNOWLEDGEMENTS

I would like to thank my committee chair, Eric Scorsone, for teaching me firsthand about local government in Michigan. I would also like to thank my other committee members, Mark Skidmore, Robert Myers, Leslie Papke, and Ronald Fisher for their help and guidance throughout this process.

Thank you also to others who provided feedback on these essays: Quentin Brummet, Timothy Hodge, Sung Hoon Kang, Nicky Mason, Todd Pugatch, Gary Solon, Brian Stacy, and seminar participants at Michigan State University and the University of Michigan. Thank you to those at the Federal Reserve Bank of Cleveland for their feedback during my fellowship there, particularly Dionissi Aliprantis, Timothy Dunne, Daniel Hartley, and Francisca Richter.

Special thanks to Scott Crofoot, Jeff Klopcic, Dan Sherman and others from the City of Saginaw for providing the data for the first essay. Thanks also to city government representatives throughout the United States who provided information about the history of their residency laws through searches of their archived charters and ordinances.

Thanks to previous professors who sparked my interest in economics and public service: Father Turner, Siddharth Chandra, Shanti Gamper-Rabindran, and many more.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES.

CHAPTER 1: THE EFFECT OF VACANT BUILDING DEMOLITIONS ON CRIME UNDER DEPOPULATION

Abstract

The United States Government spends millions of dollars a year on vacant building demolitions. However, no clear causal link has been established between vacant building demolitions and crime, nor has it been determined whether demolitions affect crime globally or merely displace it into other areas within a city. I examine these questions using block face level monthly panel data from the city of Saginaw, Michigan. Because the permitting process for a demolition induces a change in crime, I do not compare crime after the demolition to crime before the demolition, but rather crime after the demolition to crime before the permit is issued. I also analyze the displacement and spillover effects of demolitions and permits on neighboring block faces through a spatially lagged independent variable model. Interestingly, results indicate that both demolitions and permits actually increase crime rather than reduce it; demolitions increase both overall crime and property crime on the block face of the demolition and permits increase property crime on the block face of the permit. Some negative spillovers do occur onto contiguous blocks, but these effects are not persistent over time. These results imply that by the time a building reaches the point of demolition, it may be too late to reverse the crime trends incurred by neighborhood decline.

Introduction

The durability of housing has been found to be the primary reason why urban decline is more persistent than growth.[1] Because houses are not removed as quickly as they are built, a negative shock decreases housing prices more than it decreases population which leads to a slower change than does a positive shock. Because of this, decline is much longer and slower than growth. Demolitions may be one way to counteract this durability and persistence. However, little research exists on the effects of demolitions or on vacancies in general.[2]

Policy makers and academics suggest that crime is related to both building vacancies and depopulation.[3] Not only does crime cause depopulation and vacant structures,[4] but vacant structures cause crime through increased incidences of arson, the sheltering of criminals, and the creation of general disorder.[5] With this as one justification, the United States Government spends millions of dollars a year demolishing vacant buildings.[6] Between 2008 and 2011, the US Department of Housing and Urban Development spent almost $200 million on vacant building demolitions under the Neighborhood Stabilization Program which is only one of several

[1] Glaeser and Gyorko, 2005

[2] Schilling and Logan, 2008

[3] In this paper, I use the term "vacant" to refer to blighted or dangerous buildings. Not all of the buildings that are demolished are technically vacant. Some still have residents living in them but are dangerous enough for the city to have the power to demolish.

[4] Cullen and Levitt, 1999

[5] Winthrop and Herr, 2009; Spelman, 1993; Eastern Pennsylvania Organizing Project and the Temple University Center for Public Policy, 2001; The Community Research Partners and Rebuilt Ohio, 2008

[6] Demolitions are also justified as a means of increasing surrounding property values, removing safety hazards, and attracting residents and businesses to the neighborhood.

funding sources for demolitions.[7] The city of Flint, Michigan alone was awarded over $3 million in 2010 -- the same year in which the number of murders in the city reached an all-time high. The city budget was so constrained that year that the jail was shut down and as a result, police officers had to issue tickets rather than arrest warrants for many offenses.[8] Are demolitions the best use of funding in such economically and fiscally stressed cities?

In this paper, I seek to answer part of this question by examining the relationship between demolitions and crime. Although previous research has shown that high-rise public housing demolitions reduce crime,[9] no research has examined the link between vacant building demolitions and crime. Vacant building demolitions differ from public housing demolitions in that they do not redistribute concentrations of low income residents as do public housing demolitions. Therefore, it is unclear whether crime is similarly affected by vacant building demolitions.

I fill this gap in the literature by estimating the temporal and spatial effects of vacant building demolitions on crime. To do this, I construct a unique data set of demolitions and crime in Saginaw, Michigan and convert it into block face observations, or houses that share a street face rather than a traditional city block. Because the permitting process for a demolition induces a change in crime, I do not compare crime after the demolition to crime before the demolition, but rather crime after the demolition to crime before the permit is issued. In order to check for spikes in crime before the permit issuance as well as changes in crime induced by the permit process, I estimate both a contemporaneous model that provides the mean effect of permits and

[7] United States Department of Housing and Urban Development, 2011

[8] For more serious offenses, Flint police officers use the county jail if there is space.

[9] Hartley, 2010

demolitions on crime as well as a flexible model that includes lags and leads of both permits and demolitions. I also analyze the displacement and spillover effects of demolitions and permits through a spatially lagged independent variable model.

Results indicate that both demolitions and permits actually increase crime rather than reduce it. One demolition increases all crime by 15% per month and property crimes in particular by 25% per month from the time of the demolition to the end of the sample period. Demolition permits increase property crimes by 20.4% per month on average from the time the permit begins to the time the demolition occurs. Demolitions on contiguous block faces do not induce a statistically significant mean change in crime on surrounding blocks, indicating that no mean contiguous spillover effects are occurring. Some spillovers and displacements can be seen in the dynamic model, but these effects are no persistent over time. Results are robust to a number of different specifications including varying numbers of leads and lags and the exclusion of multiple demolitions.

These results show that, in the short run, vacant building demolitions do not reverse the crime levels and trends of declining neighborhoods. This is consistent with social disorganization theory which suggests that social capital and cohesion are disrupted when a neighborhood loses population and the social controls that put limits on criminal activity deteriorate.[10] Demolitions are not likely to reverse this effect since they do not replace these social controls. Results may also be consistent with an induced change in the perceived and/or actual probability of apprehension when a house is demolished. The actual probability of apprehension increases when there is no shelter to house a crime, and the perceived probability

[10] Park and Burgess, 1925; Shaw and McCay, 1932, 1942, 1969

decreases due to the appearance of fewer eyes on the street. Both of these effects may cause an increase in crime.

In their report on the cost of vacant and abandoned properties, The Community Research Partners and Rebuild Ohio argue that vacancies create a downward spiral for neighborhood housing markets that is difficult to correct, even with large infusions of public dollars.[11] By the time a house gets to the point of demolition, it may be too late. Earlier interventions such as foreclosure prevention may be more successful at reducing crime and neighborhood decline.

Conceptual Framework

There are three theories that lend insight into how demolitions may impact criminal behavior.[12] First, the rational choice theory of crime hypothesizes that criminals maximize their economic well-being by comparing the benefits and costs of crime such as fines, imprisonment, and social stigmatism.[13] If the potential gain from committing a crime is sufficiently greater than the combined risk of being caught and the size of the punishment, then the criminal chooses to commit the crime. A demolition may induce a change in the actual or perceived probability of being caught committing a crime due to the removal of the structure. The actual probability of being caught committing a crime may increase after a demolition due to the lack of a shelter to hide the crime. This may increase reported crime on or near the vacant parcel. The perceived probability of being caught committing a crime may decrease because it may appear that the number of eyes on the street has been reduced. This may also lead to increased levels of crime

[11] The Community Research Partners and Rebuilt Ohio, 2008

[12] For an excellent review of the crime literature, see Deller, Amiel, and Deller, 2011

[13] Fleisher, 1963, 1966a, 1966b; Becker, 1968a, 1994; and Ehrlich, 1973, 1975

on a block face that has undergone a demolition. However, once a house has been demolished, there is less property to steal or burn, reducing the benefits of crime, so property crime might also decline. This theory does not provide a clear prediction of the sign of the effect of demolitions on crime.

These changes in the perceived costs and benefits may cause two types of crime displacement. First, a criminal may choose to commit a different type of crime because he can no longer undergo the activity that he had previously undertaken. [14] For example, if a drug dealer can no longer produce a drug in an abandoned building, he may switch to robbery as a source of income. I examine this displacement by categorizing crimes into different groups and examining their changes before and after a demolition. Crime displacement may also occur after a demolition if a criminal moves to another location in order to commit the same crime. For instance, a drug producer may simply move to another vacant building to use as a laboratory. I examine these displacement effects through a spatially lagged independent variable model.

The second theory that relates demolitions to criminal behavior is what has been termed the 'broken windows' theory.[15] This theory states that if a window in a building is broken and left unrepaired, the rest of the windows in the building will soon be broken as well. Window breaking does occur on a large scale because some areas are inhabited by determined window-breakers whereas others are populated by window- lovers. Rather, one unrepaired window is a signal that breaking more windows costs nothing. In the case of vacant buildings, the theory implies that one vacant building lying decrepit leads to further crime solely based on the signal that the probability of being punished is low. This suggests that demolitions cause positive

[14] Repetto, 1974

[15] Kelling and Wilson, 1982

spillover effects in which crime is reduced not only in the immediate area but in surrounding areas as well. However, this theory is highly disputed. Some authors have found that property disorder does increase crime and that targeting this disorder is a viable crime reduction strategy,[16] while others find no support for the broken windows theory nor for the proposition that broken windows policing is the optimal use of scares law enforcement resources.[17]

The third theory that lends insight into the relationship between demolitions and crime is the social disorganization theory of crime. This theory suggests that social capital and cohesion are disrupted when a neighborhood loses population and the social controls that put limits on criminal activity deteriorate.[18] As houses become vacant, crime increases. Previous research has found this to be true with estimates ranging from $1,472 of public safety money spent per vacant property[19] to a doubling of crime rates on block faces with open abandoned buildings.[20] A number of studies look at foreclosures rather than at vacant buildings with most finding a positive relationship between foreclosures and crime[21] and some finding no relationship between the two[22]. However, demolitions are not likely to reverse this effect since they do not replace these social controls.

---

[16] Braga and Bond, 2008; Braga et. al., 1999; Corman and Mocan, 2005; Sousa and Kelling, 2002

[17] Harcourt and Ludwig, 2006; Fagan and Davies, 2000

[18] Park and Burgess, 1925; Shaw and McCay, 1932, 1942, 1969

[19] Winthrop and Herr, 2009

[20] Spelman, 1993

[21] Teasdale, Clark, and Hinkle, 2012; Ellen, Lacoe, and Sharygin, 2011; Clark and Teasdale 2005; Stucky, Ottensmann, and Payton, 2012; Arnio, Baumer and Wolf, 2011; Goodstein and Lee, 2010; Acevedo, 2009; Pandit, 2011; Harris, 2011; Immergluck and Smith, 2006

[22] Cui, 2010; Madensen, Hart, and Miethe, 2011; Jones and Pridemore, 2012; Kirk and Hyra, 2011

In addition to the demolition, the permitting process for a demolition affects crime as well. Anecdotal evidence suggests that permits increase property crime because criminals patrol permitted structures based on public records and permit stickers on the houses.[23] These individuals then strip vacant and nearby houses of copper pipes, fixtures, and aluminum siding.[24] This change in crime during the permit process may cause persistence in crime in that may extend into the post demolition period; once a criminal targets a neighborhood, he may be more likely to return later. However, it is also possible that the permitting process decreases violent and other types of crime because of the frequent entrance and exit of city officials into the house and neighborhood.

These theories do not provide a clear hypothesis as to how demolitions will affect crime. It is possible that both the permit process and demolitions either increase crime or decrease crime not only on the block face of the permit or demolition but on surrounding block faces as well.

Data

The data for this study were collected in collaboration with the city of Saginaw's geographical information systems (GIS) and inspections departments. The GIS department provided block level demographics and parcel level crime data and the Inspections department provided detailed demolition data, including permit dates, demolition costs, and sources of funding. I then converted these data into block face level observations to better capture neighborhood interactions as discussed in more detail later in this section. These data provide a unique opportunity to examine the impact of permits and demolitions at a detailed level.

[23] Personal correspondence with Thomas Fitzpatrick, Federal Reserve Bank of Cleveland

[24] The Community Research Partners and Rebuilt Ohio, 2008

The data set consist of 2,443 block faces spanning 24 months: January 2008 through December 2009. Table 1.1 displays basic summary statistics for crimes, demolitions, and permits on these block faces. Figure 1.1 illustrates the data geographically. Both maps show the density of vacant parcels in Saginaw in 2009 in the underlying shades of the chloropleth (the intensity of the color indicates the degree of vacancy). The first map shows the demolitions that took place in 2008 and 2009 and the second shows all crimes that took place in those same years.

I break incidents of crime into three categories: all crime, violent crime, and property crime. Violent crime includes murder and non-negligent manslaughter, forcible rape, robbery, simple assault, and aggravated assault. Property crime includes burglary and breaking and entering, purse snatching, theft from a building, theft from a machine, theft from a vehicle, other larcenies, motor vehicle theft, stolen property, destruction of property, shoplifting, pocket picking, and arson. Violent crime and property crime do not sum to all crime; there are some crimes that are not counted as either type such as fraud, intimidation, weapon law violations, and drug/narcotic violations.

Figure 1.2 displays the number of demolitions and permits that occur each month in my data set. Only 13 block faces have more than one demolition in a month during the time period of my data set. This breakdown can be seen in Table 1.2. Thirty eight block faces have more than one cumulative demolition over the entire time period which can be seen in Table 1.3. The length of time that a house is permitted for a demolition before it is demolished varies from 0 to 23 months with a mean of 11.33 months. The frequency of permit times can be seen in Figure 1.3.

In order to more accurately model the impact of demolitions on crime, I convert the data into block face observations rather than block level observations. A block face is all parcels that

face each other on a street segment rather than all parcels on a traditional city block as defined by the census. A traditional city block includes all houses on either side of a block rather than houses that face each other on a street. An example of a traditional city block and a block face can be seen in Figure 1.4. The mean number of parcels on a block face is 18.74. A block face more accurately captures the likely effect of a demolition on crime since it is logical that an event on one side of the street has a greater impact on a house across the street than it does on a house on the other side of the block.

Brief Background on Saginaw

Saginaw has undergone rapid depopulation since the decline of manufacturing in the latter half of the twentieth century. Foreclosures and land vacancies that had already been increasing were exacerbated by the financial and housing crises of 2007 which is illustrated in Figure 1.5. Crime is also a serious problem. As mentioned previously, Saginaw ranked as the most violent city in America from 2003 through September of 2008.[25]

Property values have decreased substantially from an already depreciated level since the financial crisis of 2007, as illustrated in Figure 1.6. Saginaw home prices devalued by 10.3% in 2009 and the unemployment rate reached 19.7%.[26] Because of the lag between the time when a home loses value and when its official assessed value decreases, assessed property values will continue to feel the effect of both the recession and depopulation long after they have both

[25] Burns, 2010
[26] Sperling's, 2009

subsided.[27] Because of these issues, the municipal consulting firm of Plante & Moran projects that the city of Saginaw could face a $19.9 million deficit by 2014 if leaders do not adjust to declining revenues and a shrinking population.[28]

Demolition Process

Demolitions are a key part of Saginaw's policy relating to vacant and abandoned buildings. To implement demolitions, Saginaw uses both Community Development Block Grant (CDBG) and Neighborhood Stabilization Program (NSP) funds provided by the US Department of Housing and Urban Development (HUD). The buildings that Saginaw demolishes need not be vacant; they only need to be qualified as structurally dangerous. They could be owned by private individuals, corporations, limited liability companies, mortgage companies, the county treasurer, or the county land bank. The city does not take ownership of the property; it only enforces its right and obligation to keep the city safe. Once demolished, the property value drops to the current local value for a vacant lot of equal size.

---

[27] Since the approval of the General Property Tax Act in 1893, property taxes have been the main source of revenue for local governments in Michigan. However, the property tax structure was altered in 1994 by Proposal A which placed a constitutional cap on the growth of taxable value (TV). Since Proposal A was instated, the TV of a property has been allowed to increase only by the lesser of the rate of inflation or five percent until the property is transferred (not including additions or new construction). Historically, this value has been below the state equalized value (SEV) which has led to a general decline in property tax revenues as a proportion of property values. It was not anticipated that the SEV would begin to decrease and eventually fall below TV as has already occurred for some properties throughout the state. When this happens, the property tax paid by the owner follows the fall in SEV. In the short-run, Proposal A may help to insulate local revenues from the declining home values. However, when house prices do stabilize and begin to increase, TV will be ratcheted downward and local unit fiscal capacity may not recover for years.

[28] Engel, 2010

Once a building is put on the dangerous building list, it is demolished in numerical order. There are three ways in which a property can be placed on the dangerous building list. First, a building can be added to the list through a resident complaint filed with the city's complaint department or clerk's office. Second, a building can be added due to an internal complaint from a city worker who observed the property firsthand while in the field or received complaints at a neighborhood meeting. Third, a building can be added through citywide sweeps undertaken by the inspections department each spring and sometimes in the fall.[29]

The first two methods of adding a house to the dangerous building list may systematically correlated with crime since it is likely that residents are more likely to complain about a property if there is crime occurring in or near it and city workers may be in the neighborhood because of crime. I inspect for spikes in crime before permit issuance by including leads of the permit variable in my dynamic specification and find no evidence of this effect.

If a building undergoes arson, it is immediately demolished. I use this information to examine the endogeneity of demolitions and arson within Saginaw. I also exclude the 25 emergency demolitions and the arsons that caused them from my dataset because they provide a direct source of endogeneity. Although this is a concern for the analysis in that it may be causing me to miss an important crime reduction caused by demolitions, there is additional concern that the structure of the demolition policy is incentivizing residents to commit arson against neighboring vacant homes because if a vacant building undergoes arson, it is moved to the top of the demolition list. If I include these emergency demolitions and arsons in my analysis I am not able to determine whether the demolition policy causes the arsons, whether the arsons cause the demolitions, or what I would truly like to know: whether demolitions reduce the

[29] Personal correspondence with Scott Crofoot, Dangerous Buildings Inspector, City of Saginaw

incidence of arson. I do include all other arson that does not immediately lead to a demolition which allows me to determine whether demolitions reduce the incidence of arson in the neighborhood of the demolition. In addition, I perform robustness checks that include and exclude all arson to determine whether they cause bias in my results.

The demolitions process follows a set procedure. When the house becomes permitted for a demolition, a notice to the owner is placed at the front entry of the building as part of the notification requirements. This occurs once at the beginning of the process and again 75-80 days later when the notice of findings is posted. Fifteen to thirty days after the last posting, the house is measured to provide cubic volume and determine demolition costs as well as inspected for asbestos.[30] This activity near the end of the permit period is believed to cause an increase in theft of anything that has a metal content.[31]

Table 1.4 lists crimes that occurred before permit issuance on parcels that underwent a demolition and Table 1.5 lists crimes that occurred during permit issuance on parcels that underwent a demolition. The most common crime in both lists is arson, followed by burglary, larceny, and damage to property. Surprisingly, there are no incidences of drug crimes on these vacant properties. Table 1.6 lists crimes that occurred on parcels after a demolition took place. Surprisingly, there were still 4 arsons even after the vacant building was removed. These are likely arsons in which brush or garbage was lit on fire.

[30] The City of Saginaw does not board up houses. Any boarding that occurs is either completed by the owner or by volunteers during an event that takes place each year before Halloween. If the city did systematically board up vacant houses it would likely affect the incidence of crime on that parcel.

[31] Personal correspondence with Scott Crofoot, Dangerous Buildings Inspector, City of Saginaw

Empirical Methodology

To estimate the causal effect of a demolition on crime, I would like to compare a block face that underwent a demolition to itself had it not undergone the demolition. This, unfortunately, is not possible. Alternatively, comparing a block face that underwent a demolition to one that did not will be misguided since there may be unobserved factors that make block faces with demolitions different from block faces without demolitions, which can be seen in Tables 1.7 and 1.8.[32] This leads to inconsistent estimates if using a cross section for analysis.

Estimates that do not control for month fixed effects also lead to misleading results since a large portion of the demolitions occurred in the winter when crime levels are the lowest. Table 1.9 displays the average number of crimes before a permit, during the permit period, and after a demolition on the block faces in the city that had a demolition during the data period. The permit period is associated with a slightly higher average level of total crime and property crime than the pre-permit period, and no mean difference in violent crime. The post demolition period is associated with a slightly lower level of total crime than the pre-permit period and a slightly higher level of property crime, with no difference in violent crime. However, these results likely reflect the monthly seasonality of crime. Figure 1.7 plots the average monthly number of crimes before and after a demolition. It appears from this graph that average crime levels are declining prior to a demolition and then remaining fairly steady after a demolition. However, this also is most likely a reflection of seasonality.

I therefore use a within estimator to compare block faces that underwent a demolition to themselves over time which removes the time invariant unobserved heterogeneity of each block face. However, I do not compare crime after a demolition to crime immediately before a

[32] Note that demographic data are only available at the block level (not at the block face level) so these means are for the block rather than block face.

demolition because the permit process for a demolition induces a change in crime that biases results. If the permit process increases property crime which appears to be the case both in the data and anecdotally, using the permit period as the base period will bias coefficients downwards. Alternatively, violent and other crime may actually decrease during the permit period due to city officials entering and leaving the premises. This will cause these types of crime to appear to increase after a demolition when in fact they are reverting to the mean.

In order to examine and account for these trends, I estimate both a contemporaneous model with the variables specified as stock variables and a dynamic model with the variables specified as flow variables. The stock of demolitions is the cumulative number of demolitions that have occurred on the block face and the stock of permits is the cumulative number of houses with permits that have not yet undergone a demolition. The flow variable for each is defined as the first difference of the stock variable, i.e.:

$$DemosFlow_{i,t} = DemosStock_{i,t} - DemosStock_{i,t-1}$$

$$PermitsFlow_{i,t} = PermitsStock_{i,t} - PermitsStock_{i,t-1}$$

An example of both stock and flow variables for each variable can be found in Table 1.10.

Because my dependent variable, the number of crimes on a block face in a given month, takes on non-negative integer values (is a count variable), a linear model for $E(y \mid \mathbf{x})$ is not ideal because it can lead to negative predicted values.[33] Also, because *y* can take on the value zero

[33] Wooldridge, 2002 p 388

I use levels of crime as my dependent variable rather than the crime rate because no good time-varying estimate of population per block exists in my data set. The only number that I could use

with positive probability, a log transformation is inappropriate. Therefore, I assume that y_i given x_i takes on a Poisson distribution. The Poisson model is as follows:

$$f(y_i) = \frac{e^{-\lambda_{i,t}} \lambda_{i,t}^{\ y_{i,t}}}{y_{i,t}!} \quad \text{for } y_i = 0,1,2\ldots \qquad (1)$$

where $\lambda_{i,t} = e^{x_{i,t}\beta} = E(y_{i,t} \mid x_{i,t}) = Var(y_{i,t} \mid x_{i,t})$.[34] Provided that $E(y_{i,t} \mid x_{i,t}) = \exp(x\beta)$, estimates of β are consistent even if the mean does not equal the variance -- when there is over dispersion.[35]

I then estimate the above model using multinomial quasi-conditional maximum likelihood estimation as described in Wooldridge, 1999 using two main equations for $x_{i,t}\beta$. In the first,

---

is the number of occupied parcels on each block. However, this number is defined as the number of parcels with a building on it, not the number of parcels with a person living on it. Therefore, this number changes when a demolition occurs and would cause simultaneity in my estimation. In addition, an occupied parcel could have any number of residents on it from a single person home to a multi-family rental. Therefore a crime rate based on this number would not only be inaccurate but would also cause bias in my results.

[34] Winkelmann, 2008

[35] Fixed effects estimations in nonlinear models such as this one generally lead to inconsistent estimates. However, the Poisson distribution can be arbitrarily misspecified and any kind of serial correlation can be present and the fixed effects Poisson estimator is consistent under mild regularity conditions (Wooldridge, 2002 p 648). Provided that $E(y \mid x) = \exp(x\beta)$, estimates of β are consistent even if the mean does not equal the variance. Therefore, a fixed effects model is appropriate and over dispersion can be ignored.

I specify both permits and demolitions as stock variables and estimate the following equation:

$$x_{i,t}\beta = \alpha_0 + \delta_1 Permits_{i,t} + \beta_1 Demos_{i,t} + \theta_i + \gamma_t + u_{it} \qquad (2)$$

Each variable is measured at the block face and month level. $Permits_{i,t}$ equals the number of permits issued for block face i in month t. $Demos_{i,t}$ equals the number of demolitions on block face i in month t. The coefficient δ_1 on $Permits_{i,t}$ gives us the mean impact of permits on crime from the month of issuance to the month of the demolition. The coefficient β_1 on $Demos_{i,t}$ tells us the mean impact of demolitions on crime from the month of the demolition through the end of the sample period. Block face and month fixed effects are represented by θ_i and γ_t respectively. Time invariant variables are captured by the block face level fixed effects and are therefore not included in this equation. I use standard errors clustered at the block face level that are robust to heteroskedasticity and arbitrary forms of error correlation within each block face.

I then estimate a second equation for $x_{i,t}\beta$ which allows the effect of permits and demolitions to vary over time through use of a dynamic treatment specification. I specify the permit and demolitions variables as flow variables and include two leads and five lags of the permit variable and five lags of the demolition variable. I also include a variable that equals the sum of all demolitions that took place six or more month previously and all permits that took place six or more months previously. This is specified as follows:

$$x_{i,t}\beta = \alpha_0 + \sum_{j=-2}^{5} \delta_j Permits_{i,t-j} + \delta_6 \sum_{j=6}^{24} Permits_{i,t-j} + \sum_{j=0}^{5} \beta_j Demos_{i,t-j} + \beta_6 \sum_{j=6}^{24} Demos_{i,t-j} + \theta_i + \gamma_t + u_{it} \quad (3)$$

Variables are once again measured at the block face and month level. $Permits_{i,t-j}$ equals the number of permits issued for block face i in month *t-j*. This variable is set to zero if the block face has undergone the demolition for which this permit was issued in month *t-j*. This ensures that the permit and demolition variables are not both greater than zero at the same time, allowing me to separately identify the dynamic effects of the permit from the dynamic effects of the demolition. $Demos_{i,t-j}$ equals the number of demolitions on block face i in month *t-j*.

The δ_j coefficients indicates the cumulative effect of a permit *j* months ago on crime now, compared to 3 or more months before the permit and the β_j coefficients indicates the cumulative effect of a demolition *j* months ago on crime now, compared to 3 or more months before the permit. The second and fourth summations in equation (3) represent all permits and demolitions that occurred six or more months before month *t*. The second summation is also set to zero when the demolition for which the permit was issued occurred in month *t-j*.

To further check that I am separately identifying the effect of permits and demolitions and to find the mean effect of a demolition on crime ignoring the permit period, I estimate the

following equation with permit months dropped from the sample and demolitions specified as stock variables:

$$x_{i,t}\beta = \alpha_0 + \beta_1 Demos_{i,t} + \theta_i + \gamma_t + u_{it} \quad (4)$$

Coefficient β_1 in equation (4) provides the mean effect of a demolition on crime from the time of the demolition to the end of the sample period as compared to the pre-permit period.

Displacements and Spillovers

Knowing whether crime changes on one block face due to a demolition does not tell us whether overall crime changes or is merely displaced into surrounding neighborhoods. I therefore estimate a model in which I include a spatial lag of demolitions. In other words, I add into the models above the sum of demolitions and permits that occurred in block faces that touch block face *i*, or block face *i*'s neighbors. An example of a block face and its neighbors is illustrated in Figure 1.8.

First, I estimate the mean effects model with a spatial lag as follows:

$$x_{i,t}\beta = \alpha_0 + \delta_1 Permits_{i,t} + \beta_1 Demos_{i,t} + \delta_2 W_i Permits_t + \beta_2 W_i Demos_t + \theta_i + \gamma_t + u_{it} \quad (5)$$

I then estimate both temporal and spatial lags together as follows:

$$x_{i,t}\beta = \alpha_0 + \sum_{j=-2}^{5}\delta_j Permits_{i,t-j} + \delta_6\sum_{j=6}^{24} Permits_{i,t-j} + \sum_{j=0}^{5}\beta_j Demos_{i,t-j} + \beta_6\sum_{j=6}^{24} Demos_{i,t-j} + \sum_{j=-2}^{5}\delta_{2j} W_i Permits_{t-j} + \delta_{26} W_i \sum_{j=6}^{24} Permits_{t-j} + \sum_{j=0}^{5}\beta_{2j} W_i Demos_{t-j} + \beta_{26}\sum_{j=6}^{24} W_i Demos_{t-j} + \gamma_t + \psi_t + u_{it} \quad (6)$$

where W_i is an *1* x *I* spatial weights vector that assigns a weight of 1 to block faces that are contiguous to block face *i* and a weight of 0 to block faces that are noncontiguous to block face *i*.[36] $Permits_t$ is an *I* x *1* vector of the permits on every block face in the city. $Demos_t$ is an *I* x *1* vector of the demolitions on every block face in the city.

Results

Results indicate that, compared to the pre-permit period, one demolition actually increases all crime at a mean rate of 15% per month and property crime at a mean rate of 25% per month from the time of the demolition through the end of the sample period as can be seen in Table 1.11.[37] Permits also increase property crime at a mean rate of 20.4% per month during the permit period.

[36] Drukker, Peng, Prucha, and Raciborski, 2011

[37] These results are robust to the use of lags of demolitions and leads of permits as independent variables in the place of the contemporaneous variables.

Results from the flexible model that includes lags and leads of permits and demolitions can be found in Table 1.12 and graphically in Figures 1.9 and 1.10. The dark line graphs the δ_j and β_j coefficients and the lighter lines above and below are two standard deviations above and below these coefficients. Permits increase violent crime by 54.8% the month that the permit occurs, but this effect dies out after that first month. Permits increase property crime by 43.7% one month after issuance which then increases to a cumulative effect of and 52.2% two months after issuance. The cumulative effect of permits on property crime six or more months after permit issuance is 35.2%.

Demolitions increase all crime by 49.5% the month of the demolition which decreases to 45.2% two months after the demolition, ending at a cumulative increase of 30.9% six or more months after the demolition occurs. Demolitions also increase property crime; by 72.2% two months after a demolition and a cumulative effect of 63.8% six or more months after the demolition.

There may be concern that there is a spike in crime before permit issuance because of the ways in which houses are put on the permit list as discussed in section 3b. If this were true, using the month immediately prior to permit issuance as a baseline for analysis will lead to downward bias due to mean reversion after this spike, similar to the effects of an Ashenfelter Dip found in labor studies.[38] Figure 1.11 plots the average number of crimes before and after permit issuance. From this graph, there appears to be a slight spike in crime before permit issuance.

[38] Ashenfelter and Card, 1985
An Ashenfelter dip refers to the decline in mean earnings of participants in employment and training programs during the period just prior to participation. This dip causes bias if this period is used as a baseline because of mean reversion after the dip. This is similar to the increase in crime immediately prior to a demolition permit. If used as a baseline, the effect of permits and demolitions on crime may appear to be smaller than they actually are because they are being compared to an elevated level of crime before the permit.

However, when examining this more formally through leads of permits, the leads are not statistically significant indicating that there is no mean reversion bias occurring in my analysis.

Results from equation (4) in Table 1.13 with permit months dropped from the sample show similar results to the full sample regression: demolitions increase all crime by 17.2% and property crime by 29.8% per month. This is the mean effect of a demolition on crime compared to the pre-permit period from the time of the demolition to the end of the sample period.

Displacement and Spillover Results

Demolitions and permits on contiguous block faces do not induce a statistically significant average change in crime on the block in question as can be seen in Table 1.14. This indicates that the cumulative increase in crime that is found in the results above is not caused by crime displacement from surrounding block faces. This is not to say that crime displacement from other areas within the city is not occurring, but it is not occurring on average from geographically contiguous block faces.

When examined dynamically, however, some displacement and negative spillover effects can be seen as in Table 1.15. There is a statistically significant increase in property crime on a block face three months after a permit occurs on a contiguous block of 25.9% and 32.3% two months after a demolition occurs. This could be caused by negative spillover effects of vacant property on the nearby block, and the attraction of criminals to the area during the permitting process. Violent crime actually decreases on a block one month after a demolition occurs on a surrounding block by 39.8%. This could be caused by displacement of the violent crime from that block face to the block face with the vacant parcel.

In addition, the dynamic effect of demolitions and permits on crime on their own block changes slightly when contiguous permits and demolitions are held constant. Permits now cause an increase in violent crime of 57.6% during the month of the permit and an increase in property crime of 41.6% then up to 48.7% two months after the permit, summing to a cumulative effect of 33.2% six months or more after a permit. Holding demolitions and permits on contiguous blocks constant, a demolition on a block in question increases all crime cumulatively by 30.2% six or more months after a demolition, violent crime by 61.1% five months after a demolition, and 58.2% six or more months after a demolition.

Robustness Checks

Strict Exogeneity

If demolitions and permits are not randomly chosen but instead are partially determined by crime, $\hat{\beta}_2$ will be inconsistent due to endogeneity caused by selection bias. This appears not to be the case since the leads on the permit variable in the dynamic specification are not statistically significant. Hence, strict exogeneity is not violated.

There may still be concern that demolitions are partially determined by crime trends in a neighborhood; i.e. that demolitions are implemented in neighborhoods where crime is increasing. In order to examine whether this is true, I perform a robustness check of the model that includes time trends as follows:

$$CrimeIncidents_{i,t} = \alpha_0 + \sum_{j=-2}^{5} \delta_j Permits_{i,t+j} \times NoDemo_{i,t+j} + \delta_6 \sum_{j=6}^{24} Permits_{i,t+j} \times NoDemo_{i,t+j} + \sum_{j=-2}^{5} \beta_j Demos_{i,t-j} + \sum_{j=6}^{24} \beta_6 Demos_{i,t+j} + \theta_i + \gamma_t + \psi_{t \times i} + u_{it} \quad (4)$$

where $\psi_{t \times i}$ are block face by month fixed effects, or block face/month time trends. The coefficients are once again very similar to the model run without the time trend fixed effects and of the same sign and significance, which indicates that results are not driven by block face trends in crime.

Number of Lags and Leads

The specifications above are robust to different baseline months (leads on permits) as long as the baseline is at least two months before the permit is issued due to the spike in crime immediately before a permit. Results using a baseline of two, three, and four months before the permit are very similar to those that use two months before the permit as the baseline.[39]

When using one month before a permit as a baseline, results are biased downwards as expected due to a spike in crime immediately before a house is permitted for a demolition as discussed above. This makes it appear that crime is decreasing due to the permit when in fact it is merely reverting to the mean.

[39] Results of the robustness checks are available upon request of the author. For the sake of brevity, they are not included in this paper.

Arson

Once a house undergoes any type of fire it is immediately demolished. Because of the direct endogeneity caused by this policy, I cannot include demolitions due to arson in my analysis. There is anecdotal evidence to suggest that this demolition policy is at least in part causing arson in vacant buildings. Homeowners near vacant buildings have learned that if a vacant building undergoes arson it will be demolished more quickly, which neighbors perceive as a benefit. Therefore, even without the estimation issues caused by the endogeneity between demolitions and arson, it would be difficult to determine which incidences of arson were caused by vacant buildings themselves and which were caused by the structure of the demolition policy.

In order to deal with this issue, I first remove all demolitions that were caused by arson, and all arsons that led to demolitions from my data set. Because there may still be a concern that some arsons were undertaken in order to induce a demolition or that some demolitions were caused by arsons that were not coded properly, I do a robustness check for endogeneity bias of all regressions by removing all arsons. The results are very similar and of the same sign and significance, suggesting that this concern is not a major issue.

Multiple Demolitions

Another estimation concern may be that blocks that have more than one demolition in the same month may be biasing results because the specification assumes that two demolitions have twice the impact as one. In my sample of 2,510 block faces, 226 block faces have one cumulative demolition, only 11 blocks have two demolitions in one month, one has three demolitions in one month, and one has four demolitions in one month.

Because there are so few block faces that underwent more than one demolition in a month, there is not enough variation to separately identify separate variables for the number of demolitions, or to identify a separate squared term on demolitions. Therefore, to check this assumption, I estimate a subsample of block faces by dropping those that have more than one demolition in a month. Results of this robustness check are once again not significantly different from the baseline results, leading me to believe that this assumption is valid.

Conclusion

Millions of dollars are spent each year demolishing vacant buildings with an often cited justification that demolitions reduce crime. However, my results show that both demolitions and permits actually increase crime rather than reduce it. Compared to the pre-permit period, one demolition increases all crime at a mean rate of 15% and property crime at a mean rate of 25% from the time of the demolition through the end of the sample period. The permitting process for a demolition also increases property crime at a mean rate of 20.4%. Although these percentages seem large, these effects equate to an increase of 0.79 total crimes and 0.51 property crimes per year caused a demolition and 0.42 property crimes per year caused by a permit because the percentages are for such a small geographic area and time period.

Although there appears to be some dynamic displacement and spillover effects of crime from a demolition and permit onto surrounding block faces, the mean displacement and spillover effects are null. This suggests that crime displacement is not occurring and overall crime in a neighborhood increases when a demolition or permit issuance occurs.

These results call into to question the use of demolitions as a crime reduction tool and the methods of permitting a house for demolition. The average de-construction cost of demolishing

a house in Saginaw is $5,020.58. This does not include the overhead costs of the city demolition department or the overhead of administering the grants by US Department of Housing and Urban Development and other agencies. This money may be better spent on public safety personnel if crime reduction is the goal.

In the short run it is clear that demolitions do not reverse the crime levels and trends of declining neighborhoods. Earlier interventions such as foreclosure prevention may be more successful at impeding neighborhood deterioration and crime trends. Additional research is needed to determine whether demolitions can reverse long run neighborhood declines.

APPENDIX

APPENDIX

Tables and Figures

Figure 1.1: Density of residential vacancies in Saginaw, Michigan in 2008 and 2009 with demolitions and crimes

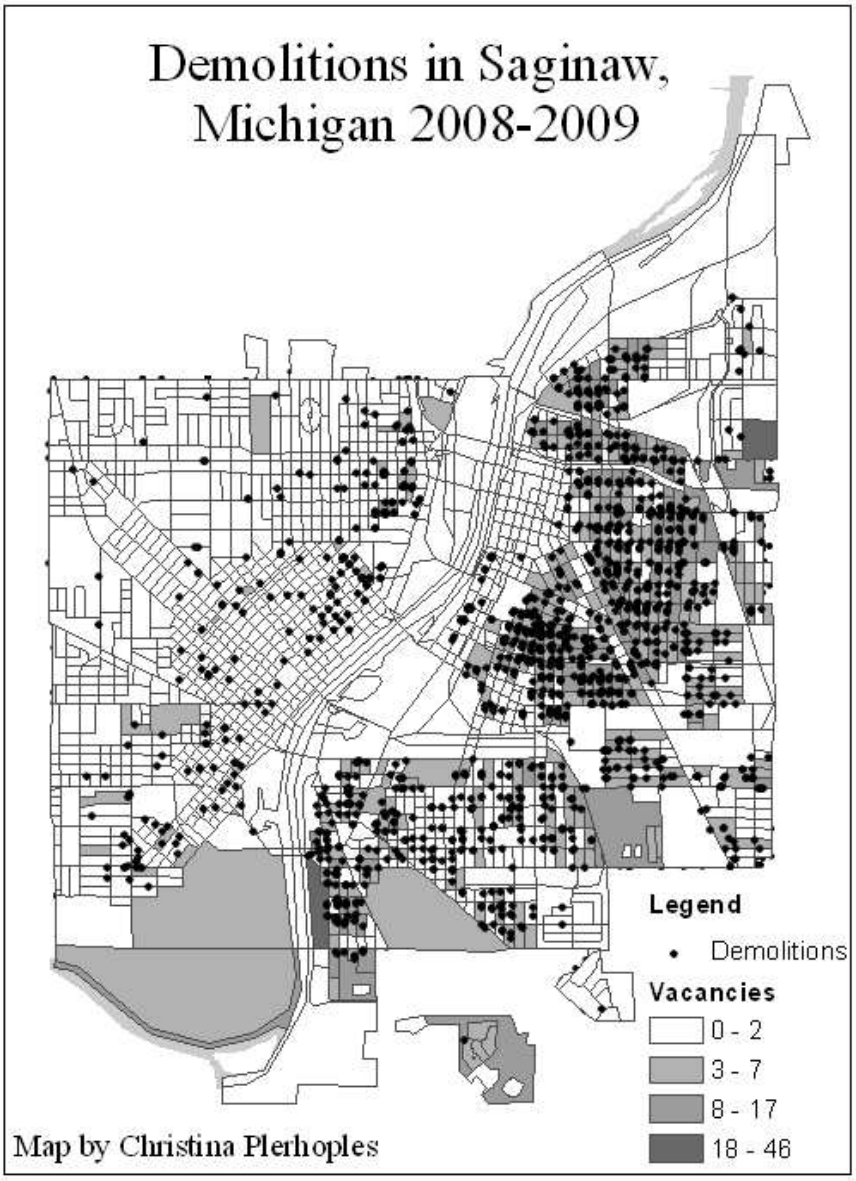


Figure 1.1 (cont'd)

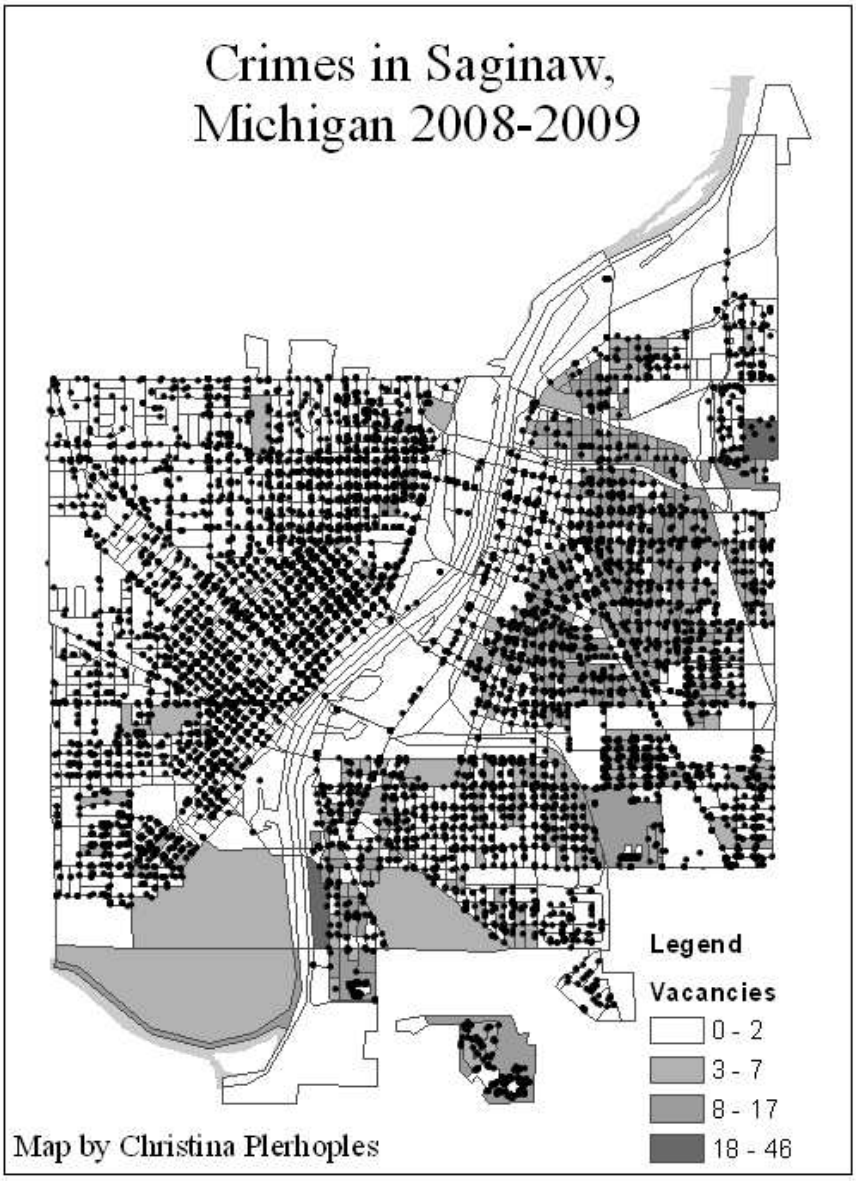


Figure 1.2: Demolitions and permits by month

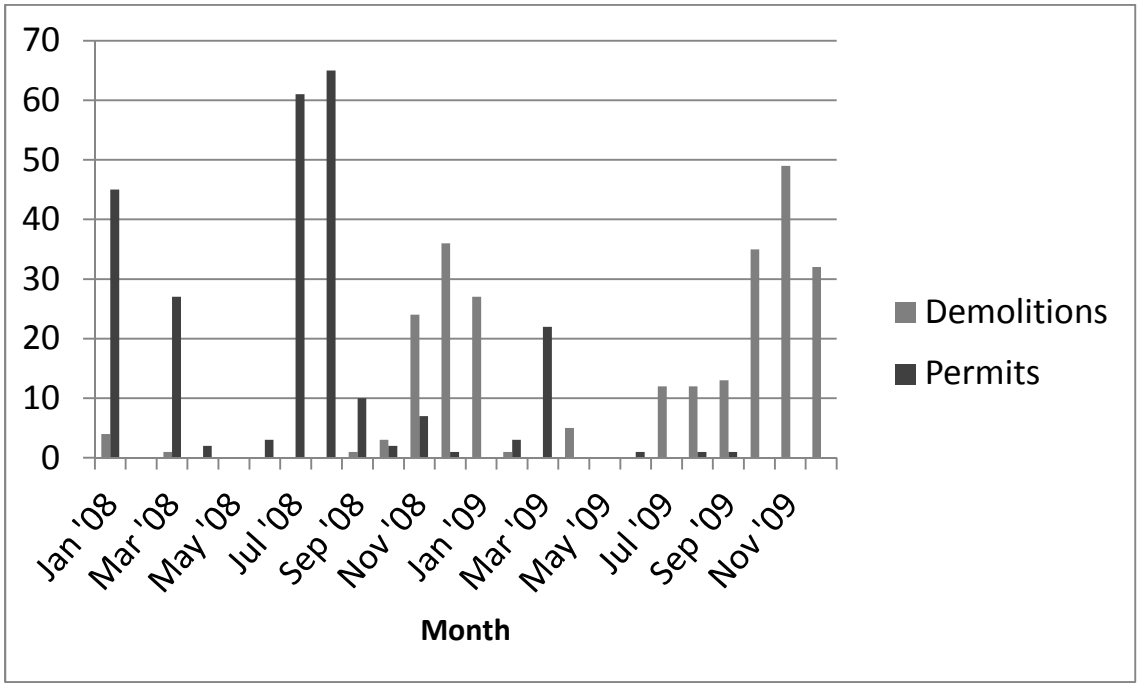


Figure 1.3: Length of time of permits

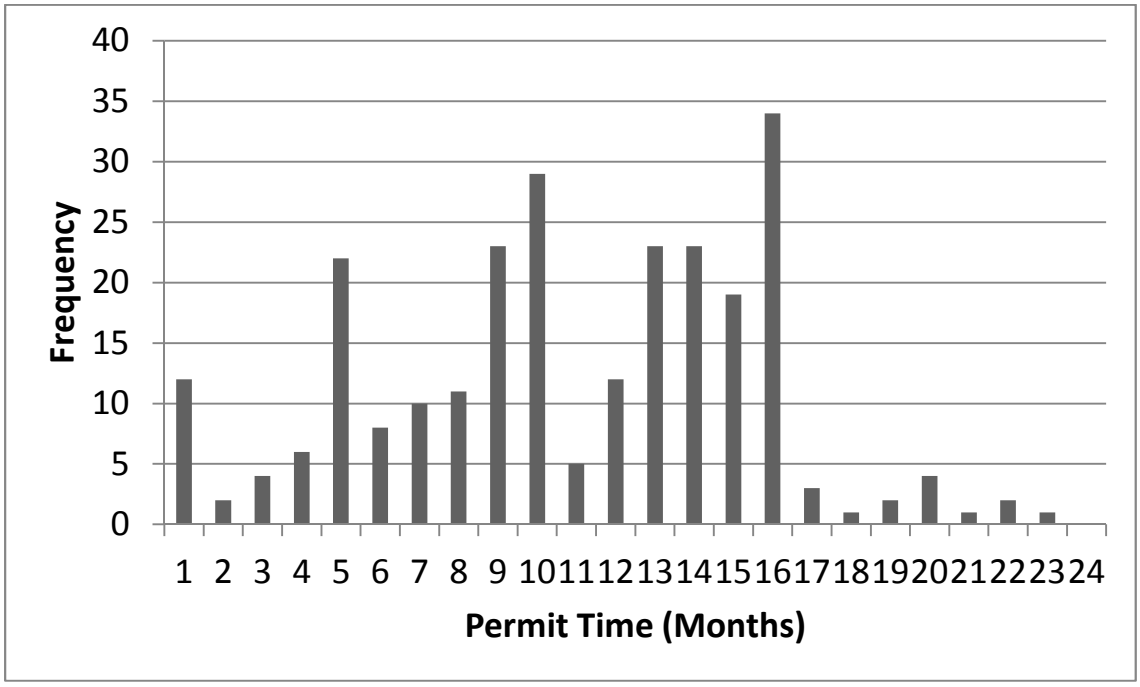


Figure 1.4: Traditional city block versus block face

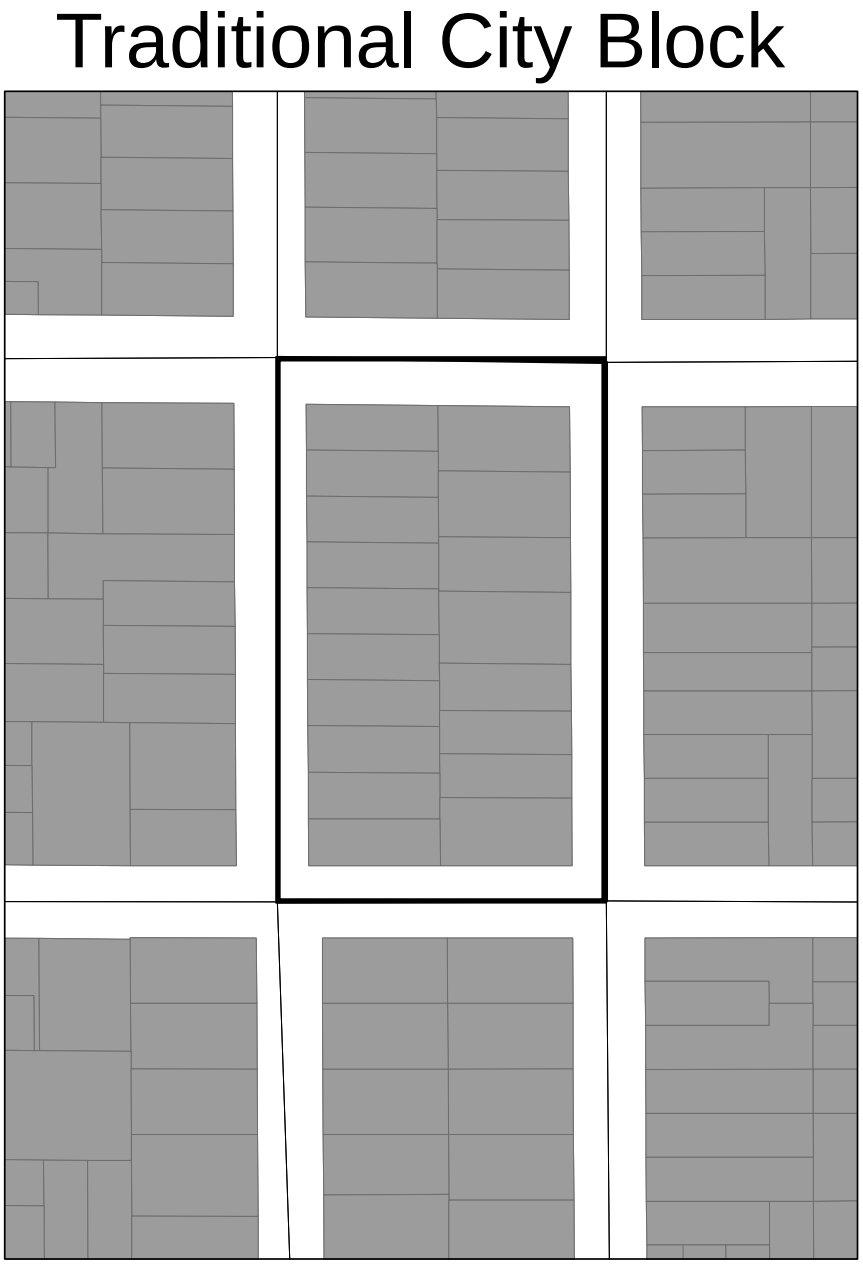


Figure 1.4 (cont'd)

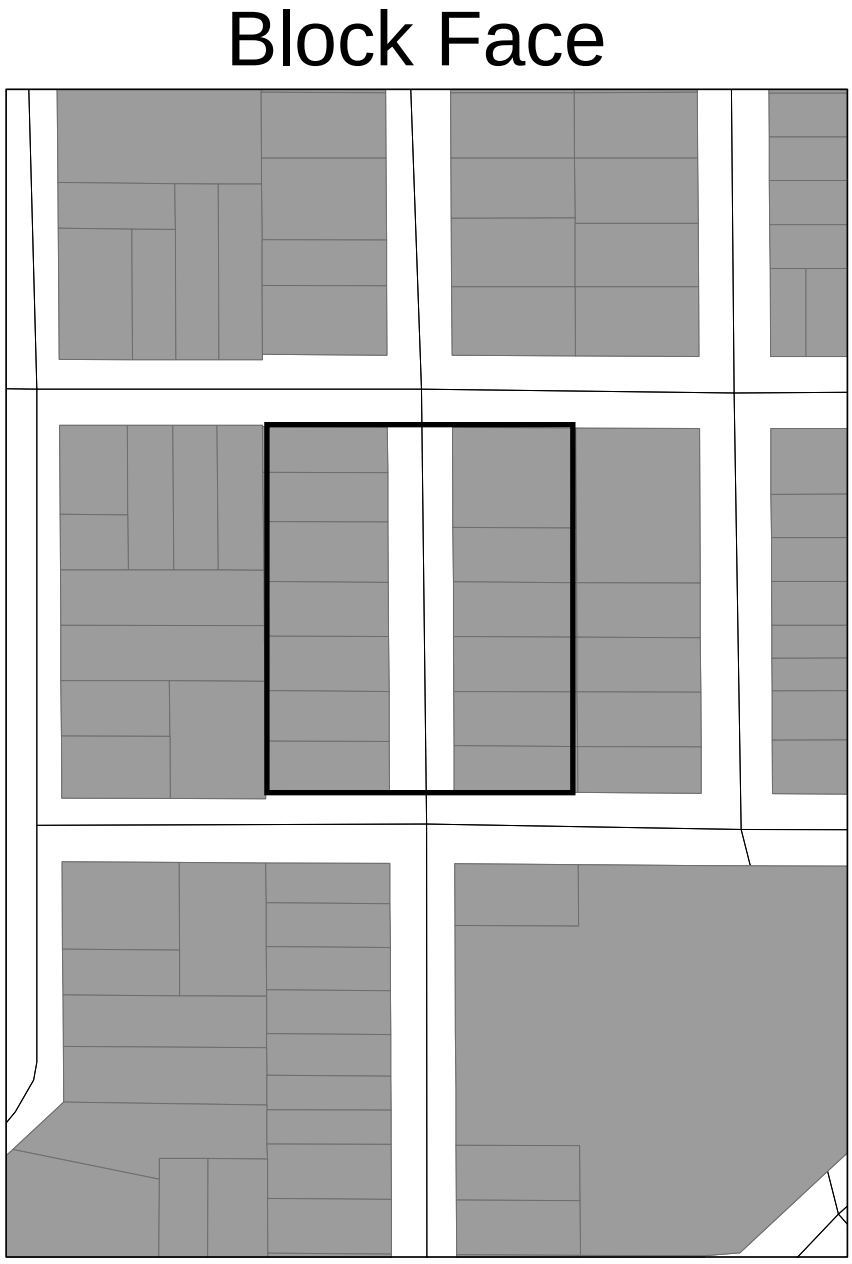


Figure 1.5: Total residential vacancies in Saginaw, Michigan, 2002-2009

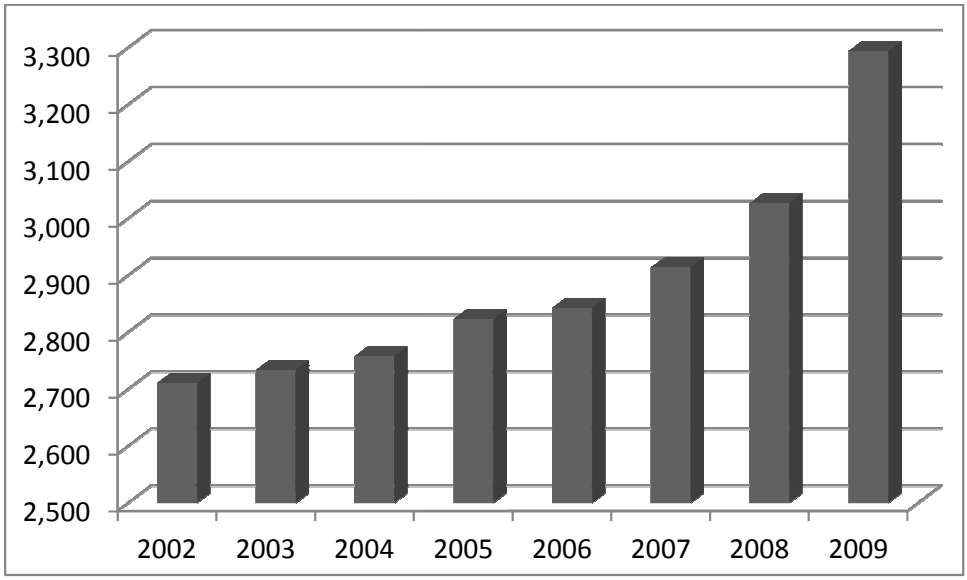


Figure 1.6: Average residential Taxable Value (TV) and State Equalized Values (SEV) in Saginaw, Michigan, 2002-2009

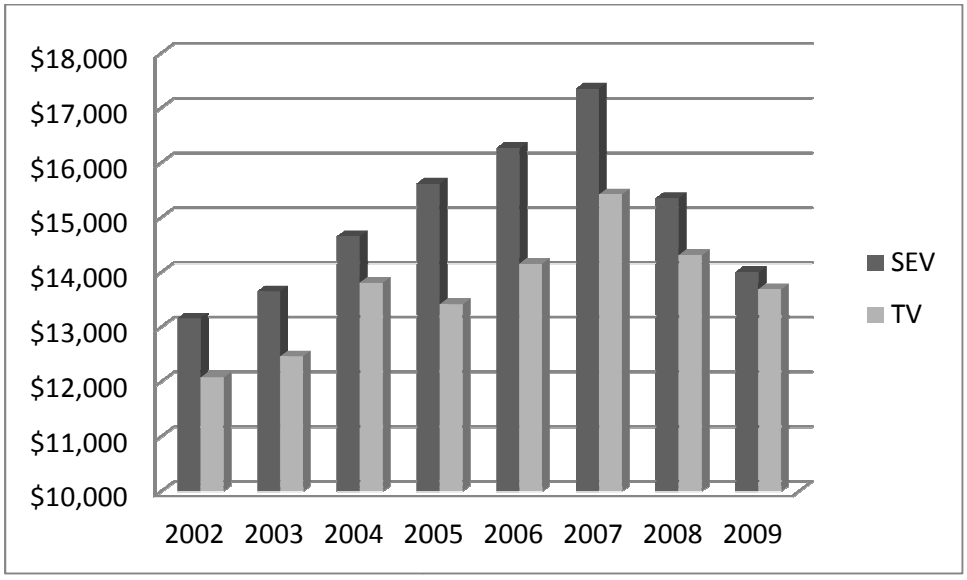


Figure 1.7: Average number of crimes on a block face before and after a demolition

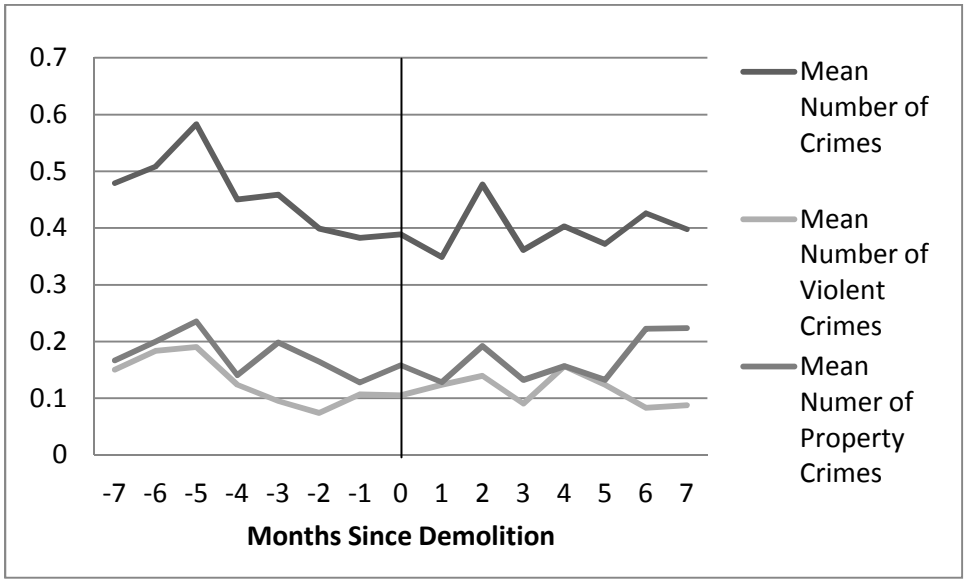


Figure 1.8: Example of a block face and its neighbors

Map by Christina Plerhoples

Figure 1.9: Dynamic effects of permits on crime

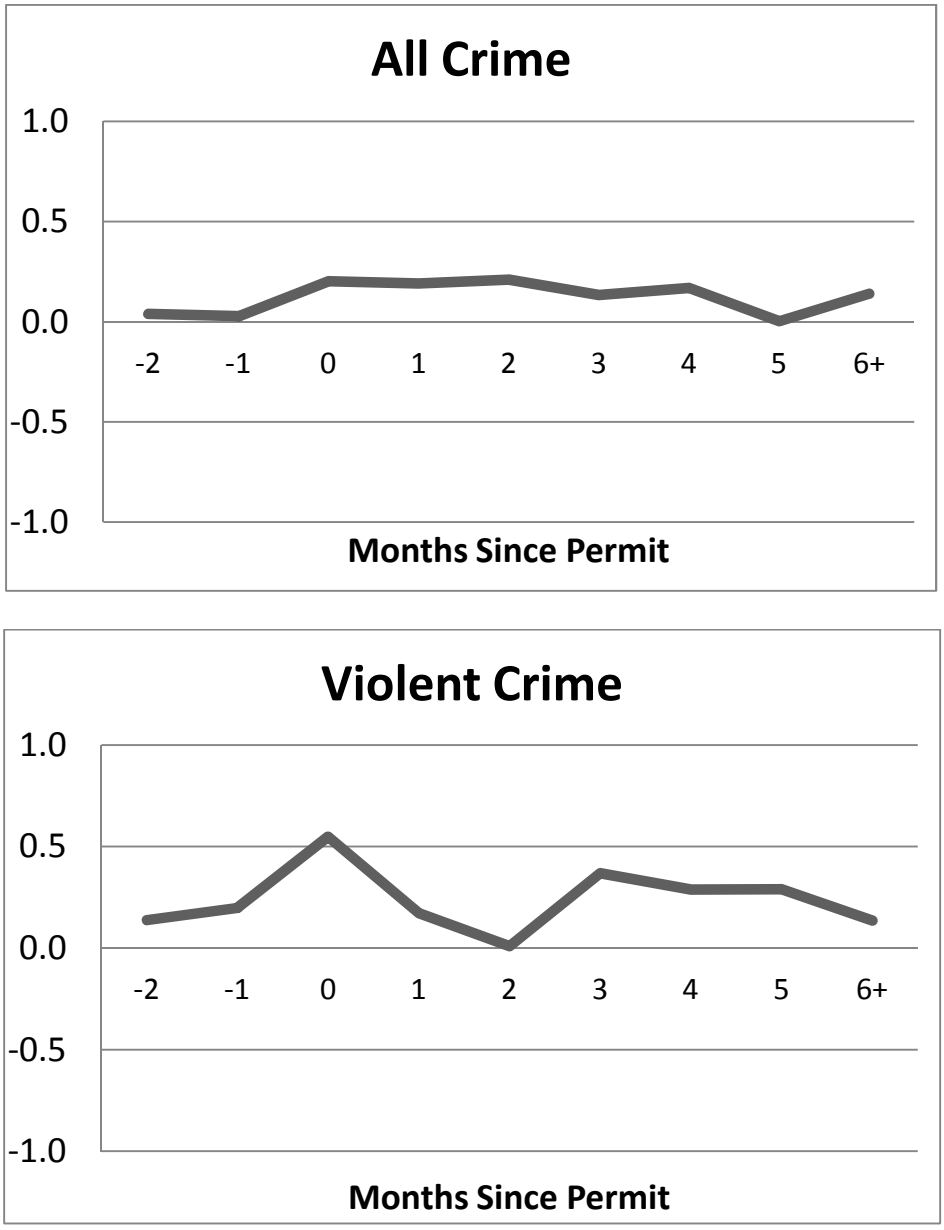


Figure 1.9 (cont'd)

Figure 1.10: Dynamic effects of demolitions on crime

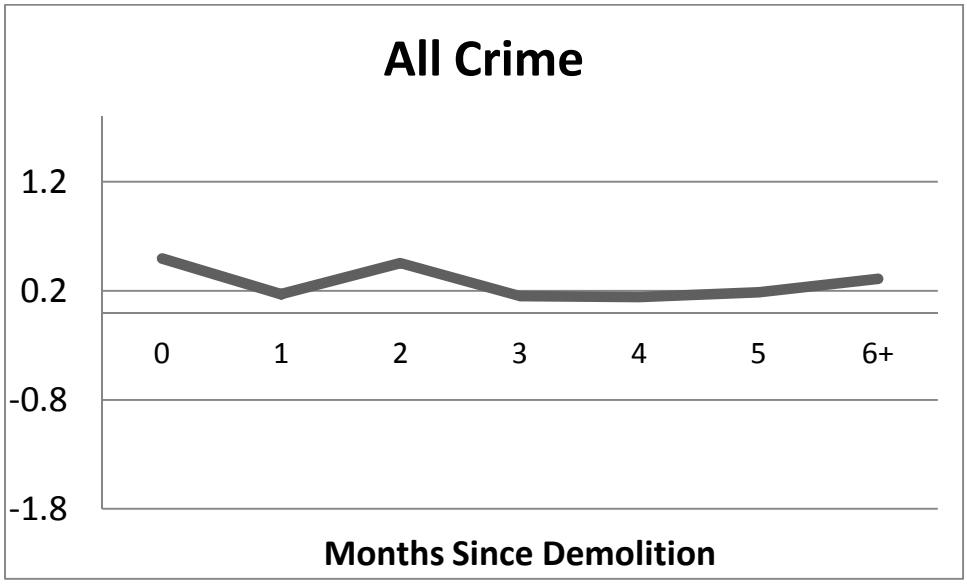
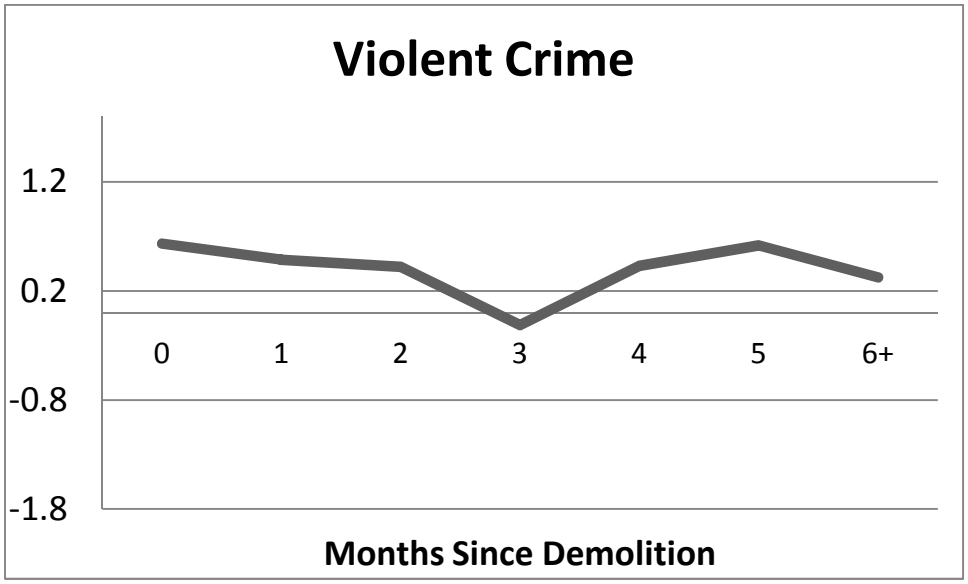


Figure 1.10 (cont'd)



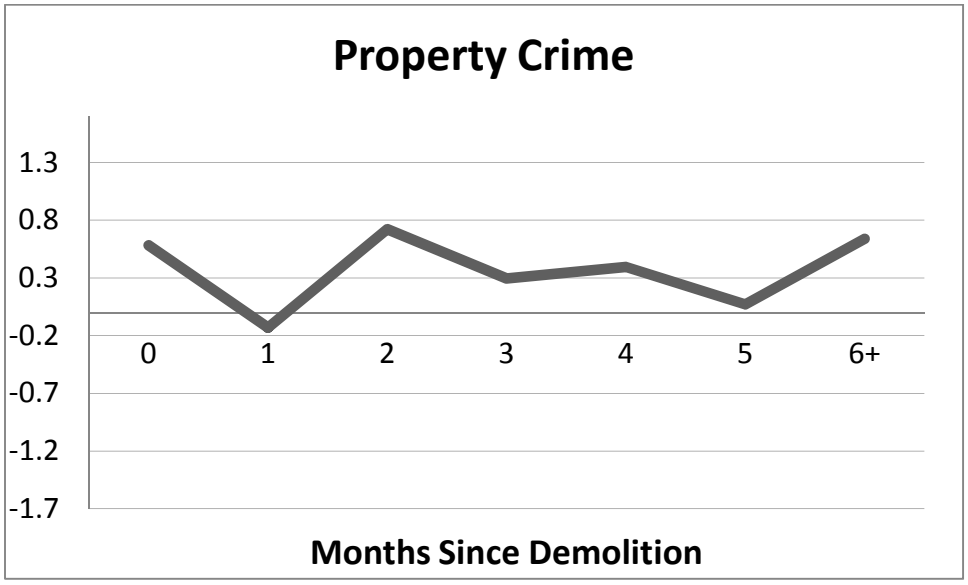


Figure 1.11: Average number of crimes on a block face before and after a permit

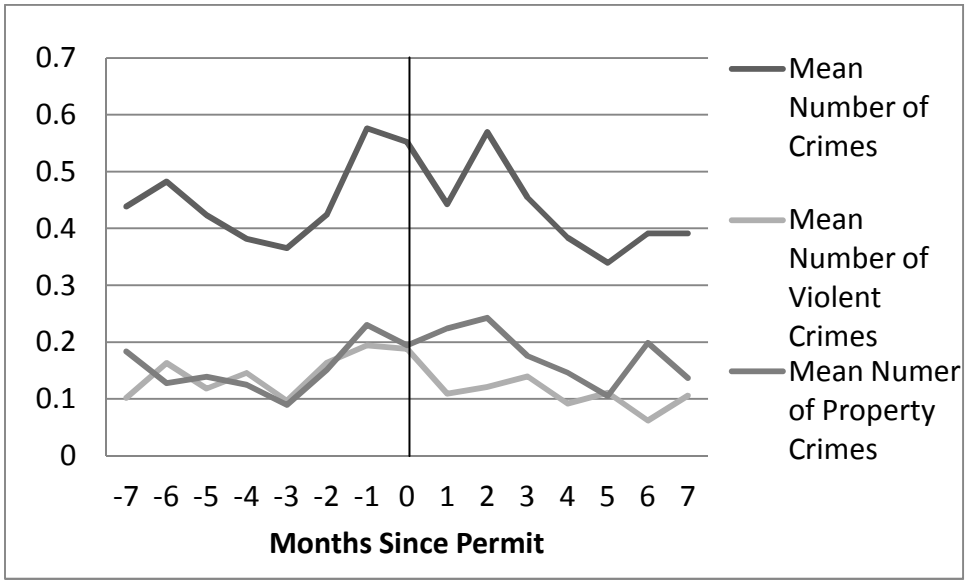


Table 1.1: Summary statistics for crimes, permits, and demolitions per block face /month

Variable (per block face /month)	No. of Obs.	Total	Mean	Std. Dev.	Min	Max
All Crimes	58,632	18,197	0.31	0.85	0	24
Violent Crimes	58,632	4,812	0.08	0.45	0	11
Property Crimes	58,632	6,973	0.12	0.48	0	15
Demolitions	58,632	255	0.00	0.07	0	4
Permits	58,632	251	0.00	0.10	0	4
Permit Time among Permitted Block Faces	251	2,789	11.33	4.43	1	23

Table 1.2: Frequency of multiple demolitions in one month

Demolitions	Freq.
0	2,204
1	226
2	11
3	1
4	1
Total	2,443

Table 1.3: Frequency of multiple cumulative demolitions over entire sample period

Cumulative Demolitions	Freq.
0	2,235
1	170
2	31
3	5
4	2
Total	2,443

Table 1.4: Crimes that occurred before a permit was issued on parcels that later underwent a demolition

Offense Description	Frequency
Arson – Residence	26
Burglary – Forced Entry – Residence	14
Larceny	6
Damage to Private Property	6
Assault and Battery/Simple Assault	3
Disorderly Conduct	3
Aggravated Assault – Non-Family -- Gun	2
Burglary – Forced Entry – Non-Residence	2
Larceny – Personal Property from Vehicle	2
Robbery – Business Strong Arm	1
Robbery – Street Gun	1
Arson (other)	1
Arson – Burning of Real Property	1
Burglary – No Forced Entry – Residence	1
Larceny – From Yards/Grounds	1
Fraud (Other)	1
Embezzlement – Business Property	1
Retail Fraud, Theft 3rd Degree	1
Disorderly Conduct (Other)	1
Traffic - Furnish False Info to Officer	1
Health and Safety Violations	1
Skipped Number	1
Inspections/Investigations - Lost and Found Property	1
Miscellaneous - General Assistance	1

Table 1.5: Crimes that occurred during the permit period on parcels that later underwent a demolition

Offense Description	Frequency
Arson – Residence	23
Larceny (Other)	5
Burglary – Forced Entry – Residence	3
Aggravated Assault\Non-Family – Gun	2
Robbery - Business Strong Arm	1
Robbery – Street Gun	1
Burglary – Forced Entry – Non-Residence	1
Burglary - No Forced Entry – Residence	1
Damage to Property – Private Property	1

Table 1.6: Crimes that occurred on parcels after a demolition

Offense Description	Frequency
Arson – Residence	4
Dog Law Violations	1

Table 1.7: Selected demographic characteristics and mean difference tests for blocks with and without demolitions

	Block faces with no Demolitions	Block faces with >0 Demolitions	Difference	P-Value
Population	32.54	39.68	-7.14	0.04
	(0.88)	(2.68)		
White	16.02	8.04	7.99	0.00
	(0.58)	(0.89)		
Black	13.37	27.50	-14.13	0.00
	(0.61)	(2.41)		
Males	15.20	17.90	-2.70	0.12
	(0.43)	(1.17)		
Females	17.34	21.78	-4.44	0.03
	(0.49)	(1.57)		
Median Age	25.77	27.14	-1.37	0.39
	(0.40)	(0.93)		
Number of Households	12.29	13.71	-1.42	0.31
	(0.35)	(0.98)		
Average Household Size	2.11	2.77	-0.66	0.00
	(0.03)	(0.08)		
Owner Occupied Housing	7.89	7.47	0.42	0.66
	(0.24)	(0.45)		
Renter Occupied Housing	4.40	6.23	-1.84	0.04
	(0.22)	(0.79)		
Vacant	1.25	2.21	-0.95	0.00
	(0.05)	(0.20)		
Size	273,795	214,886	58,909	0.39
	(17202)	(20798)		

Standard errors are listed in parentheses below the means

Table 1.8: Crime statistics and mean difference tests for blocks with and without demolitions

	Block faces with no Demolitions	Block faces with > 0 Demolitions	Difference	P-Value
All Crimes	0.237	0.274	-0.037	0.007
	(0.003)	(0.013)		
All Crimes Sans Arson	0.23	0.258	-0.028	0.035
	(0.003)	(0.012)		
Violent Crimes	0.048	0.061	-0.013	0.02
	(0.001)	(0.005)		
Property Crimes	0.117	0.131	-0.014	0.119
	(0.002)	(0.009)		
Property Crimes Sans Arson	0.117	0.131	-0.014	0.119
	(0.002)	(0.009)		

Standard errors are listed in parentheses below the means

Table 1.9: Average number of crimes on each block face in each month pre-permit, during permit, and post-demolition

	Pre-Permit	Permit	Post-Demo	Block faces with a Demo, All Months	All Block faces, All Months
All Crime	0.43	0.45	0.40	0.44	0.31
Violent Crime	0.12	0.12	0.12	0.13	0.08
Property Crime	0.15	0.18	0.16	0.17	0.12

Table 1.10: Variable Specification

Month	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24
Demos flow	0	0	0	0	0	1	0	0	0	0	0	0	0	0	0	0	1	0	0	0	0	0	0	0
Demo stock	0	0	0	0	0	1	1	1	1	1	1	1	1	1	1	1	2	2	2	2	2	2	2	2
Permits flow	0	1	0	0	0	-1	0	0	0	0	1	0	0	0	0	0	-1	0	0	0	0	0	0	0
Permits stock	0	1	1	1	1	0	0	0	0	0	1	1	1	1	1	1	0	0	0	0	0	0	0	0

Table 1.11: Effect of demolitions and permits for demolitions on crime in Saginaw, Michigan

	(1) All Crime	(2) Violent Crime	(3) Property Crime
Permits	0.085	-0.048	0.204**
	(0.070)	(0.114)	(0.093)
Demolitions	0.150**	0.157	0.250**
	(0.065)	(0.135)	(0.104)
Observations	48,504	33,120	41,808
Number of block faces	2,021	1,380	1,742

Robust standard errors in parentheses, clustered at the block face level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of all block faces in Saginaw, MI from 2008-2009. Crime offenses refer to the number of incidents on each block face in each month.

Table 1.12: Dynamic effect of demolitions and permits for demolitions on crime in Saginaw, Michigan

	(1) All Crime	(2) Violent Crime	(3) Property Crime
Two months before permit	0.038	0.137	0.152
	(0.101)	(0.225)	(0.163)
One month before permit	0.027	0.198	0.197
	(0.120)	(0.244)	(0.173)
Month of permit	0.202	0.548**	0.097
	(0.142)	(0.235)	(0.226)
One month after permit	0.192	0.172	0.437**
	(0.171)	(0.266)	(0.210)
Two months after permit	0.210	0.010	0.522***
	(0.153)	(0.277)	(0.197)
Three months after permit	0.133	0.368	0.131
	(0.175)	(0.236)	(0.238)
Four months after permit	0.169	0.288	0.358
	(0.157)	(0.295)	(0.241)
Five months after permit	0.004	0.290	-0.055
	(0.163)	(0.261)	(0.236)
Six or more months after permit	0.140	0.136	0.352*
	(0.132)	(0.218)	(0.195)
Month of demolition	0.495**	0.635	0.583
	(0.235)	(0.522)	(0.392)
One month after demolition	0.168	0.486	-0.131
	(0.227)	(0.324)	(0.426)
Two months after demolition	0.452**	0.422	0.722***
	(0.203)	(0.296)	(0.265)
Three months after demolition	0.151	-0.112	0.295
	(0.211)	(0.410)	(0.346)
Four months after demolition	0.141	0.431	0.396
	(0.216)	(0.332)	(0.325)
Five months after demolition	0.187	0.616*	0.072
	(0.223)	(0.359)	(0.294)
Six or more months after demolition	0.309**	0.323	0.638***
	(0.146)	(0.231)	(0.197)
Observations	30,400	19,072	24,560
Number of block faces	1,900	1,192	1,535

Robust standard errors in parentheses, clustered at the block face level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of all block faces in Saginaw, MI from 2008-2009. Crime offenses refer to the number of incidents on each block face in each month.

Table 1.13: Effect of demolitions on crime with permit months removed

	(1) All Crime	(2) Violent Crime	(3) Property Crime
Demolitions	0.172**	0.161	0.298**
	(0.086)	(0.156)	(0.131)
Observations	45,988	31,135	39,354
Number of block faces	2,001	1,356	1,701

Robust standard errors in parentheses, clustered at the block face level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of all block faces in Saginaw, MI from 2008-2009 with permit months dropped from the sample. Crime offenses refer to the number of incidents on each block face in each month.

Table 1.14: Displacement and spillover effects of demolitions and permits for demolitions on crime in Saginaw, Michigan

	(1) All Crime	(2) Violent Crime	(3) Property Crime
Permits	0.092	-0.042	0.201**
	(0.070)	(0.113)	(0.094)
Permits on contiguous block faces	-0.031	-0.032	0.022
	(0.032)	(0.050)	(0.047)
Demolitions	0.167***	0.172	0.256**
	(0.064)	(0.138)	(0.105)
Demolitions on contiguous block faces	-0.047	-0.052	-0.005
	(0.033)	(0.053)	(0.054)
Observations	48,504	33,120	41,808
Number of block faces	2,021	1,380	1,742

Robust standard errors in parentheses, clustered at the block face level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of all block faces in Saginaw, MI from 2008-2009. Crime offenses refer to the number of incidents on each block face in each month.

Table 1.15: Dynamic displacement and spillover effects of demolitions and permits for demolitions on crime in Saginaw, Michigan

	(1) All Crime	(2) Violent Crime	(3) Property Crime
Two months before permit	0.045	0.144	0.155
	(0.102)	(0.228)	(0.164)
One month before permit	0.036	0.201	0.202
	(0.118)	(0.237)	(0.177)
Month of permit	0.204	0.576**	0.087
	(0.143)	(0.233)	(0.228)
One month after permit	0.182	0.172	0.416**
	(0.172)	(0.266)	(0.210)
Two months after permit	0.197	0.003	0.487**
	(0.153)	(0.276)	(0.198)
Three months after permit	0.122	0.366	0.121
	(0.177)	(0.237)	(0.240)
Four months after permit	0.164	0.287	0.338
	(0.158)	(0.293)	(0.241)
Five months after permit	0.009	0.299	-0.059
	(0.162)	(0.264)	(0.234)
Six or more months after permit	0.142	0.155	0.332*
	(0.131)	(0.217)	(0.195)
Two months before permit on contiguous block faces	-0.019	-0.081	-0.044
	(0.051)	(0.083)	(0.091)
One month before permit on contiguous block faces	-0.036	-0.172*	0.079
	(0.054)	(0.100)	(0.094)
Month of permit on contiguous block faces	-0.036	-0.035	-0.040
	(0.092)	(0.137)	(0.163)
One month after permit on contiguous block faces	-0.012	-0.200	0.027
	(0.095)	(0.138)	(0.138)
Two months after permit on contiguous block faces	0.071	-0.030	0.146
	(0.086)	(0.142)	(0.139)
Three months after permit on contiguous block faces	0.076	0.007	0.259*
	(0.092)	(0.140)	(0.151)
Four months after permit on contiguous block faces	-0.013	-0.118	0.028
	(0.094)	(0.142)	(0.157)
Five months after permit on contiguous block faces	-0.015	-0.109	-0.015
	(0.095)	(0.130)	(0.155)
Six or more months after permit on contiguous block faces	-0.012	-0.188	0.126
	(0.078)	(0.125)	(0.133)

Table 1.15: (cont'd)

	(1)	(2)	(3)
	All Crime	Violent Crime	Property Crime
Month of demolition	0.489**	0.672	0.559
	(0.234)	(0.513)	(0.393)
One month after demolition	0.166	0.552*	-0.137
	(0.230)	(0.317)	(0.427)
Two months after demolition	0.416**	0.398	0.683***
	(0.200)	(0.300)	(0.264)
Three months after demolition	0.114	-0.173	0.256
	(0.215)	(0.415)	(0.348)
Four months after demolition	0.116	0.401	0.363
	(0.220)	(0.338)	(0.328)
Five months after demolition	0.188	0.611*	0.053
	(0.224)	(0.363)	(0.291)
Six or more months after demolition	0.302**	0.345	0.582***
	(0.147)	(0.226)	(0.203)
Month of demolition on contiguous block faces	-0.071	-0.239	-0.084
	(0.162)	(0.236)	(0.265)
One month after demolition on contiguous block faces	-0.051	-0.398**	0.038
	(0.105)	(0.171)	(0.170)
Two months after demolition on contiguous block faces	0.130	0.084	0.323*
	(0.101)	(0.142)	(0.194)
Three months after demolition on contiguous block faces	0.077	0.005	0.156
	(0.102)	(0.176)	(0.196)
Four months after demolition on contiguous block faces	0.043	-0.052	0.029
	(0.102)	(0.147)	(0.171)
Five months after demolition on contiguous block faces	-0.045	-0.112	-0.047
	(0.101)	(0.153)	(0.174)
Six or more months after demolition on contiguous block faces	0.007	-0.144	0.162
	(0.078)	(0.126)	(0.127)
Observations	30,400	19,072	24,560
Number of block faces	1,900	1,192	1,535

Robust standard errors in parentheses, clustered at the block face level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of all block faces in Saginaw, MI from 2008-2009. Crime offenses refer to the number of incidents on each block face in each month.

BIBLIOGRAPHY

BIBLIOGRAPHY

Acevedo, Luis G. 2009. “An Empirical Analysis of the impact of home foreclosure on the crime rate: evidence in Atlanta, GA.” *Bryant Economic Research Paper* 2 (9).

Arnio, Ashley N., Eric P. Baumer, and Kevin T. Wolff. 2010. “The Contemporary Foreclosure Crisis and U.S. Crime Rates." *Social Science Research* 41 (6):1598-1614.

Ashenfelter, Orley and David Card. 1985. “Using the Longitudinal Structure of Earnings to Estimate the Effect of Training Programs." *Review of Economics and Statistics* 67 (4).

Becker, Gary S. 1968a. “Crime and Punishment: an Economic Approach.” *Journal of Political Economy* 76 (2):169-217.

Becker, Gary S. 1968b. “Nobel Lecture: the Economic Way of Looking at Behavior.” *The Journal of Political Economy* 101 (3):385-409.

Becker, Gary S. 1964, 1994, 3rd ed. *Human Capital: A Theoretical and Empirical Analysis, with Special Reference to Education* Chicago, University of Chicago Press.

Braga, Anthony A., David L. Weisburd, Elin J. Waring, Lorraine Green Mazerolle, Willima Spelman, and Francis Gajewski. 1999. “Problem Oriented Policing in Violent Crime Places: A Randomized Controlled Experiment.” *Criminology* 37 (3):541-79.

Braga, Anthony and Brenda J. Bond. 2008. “Policing Crime and Disorder Hot Spots: A Randomized Controlled Trial.” *Criminology* 46 (3): 577-607.

Burns, Gus. 2010. "FBI stats designate Saginaw the most violent city per capita in America." The Saginaw News. MLive.com. <http://www.mlive.com/news/saginaw/index.ssf/2010/01/fbi_stats_designate_saginaw_th.html>

Clark, Lynn M., and Brent Teasdale. 2005. “Subprime Lending, Crime, and Neighborhood Disorganization: Beyond Internal Dynamics.” University of Akron.

The Community Research Partners and Rebuild Ohio 2008. “$60 Million and Counting: The Cost of Vacant and Abandoned Properties to Eight Ohio Cities.”

Corman, H. and Mocan, N. 2005. “Carrots, Sticks, and Broken Windows.” *NBER Working Papers 9062* National Bureau of Economic Research.

Cui, Lin. 2010. “Foreclosure,Vacancy and Crime.” Manuscript, Department of Economics, University of Pittsburgh.

Cullen, Julie Berry, and Steven D. Levitt. 1999. “Crime, Urban Flight, and the Consequences for Cities.” *Review of Economics and Statistics* 81 (2):159-169

Deller, Steven, Lindsay Amiel, and Melissa Deller. 2011. “Model Uncertainty in Ecological Criminology: An Application of Bayesian Model Averaging with Rural Crime Data.” *International Journal of Criminology and Sociological Theory* 4 (2):683-717.

Drukker, David M., Hua Peng, Ingmar Prucha, and Rafal Raciborski. 2011. “Creating and Managing Spatial-Weighting Matrices using the spmat Command.” <http://econweb.umd.edu/~prucha/Papers/WP_spmat_2011.pdf>

Eastern Pennsylvania Organizing Project and the Temple University Center for Public Policy. 2001. “Blight Free Philadelphia.”

Ehrlich, I. 1973. “Participation in illegitimate activities: a theoretical and empirical investigation.” *Journal of Political Economy* 81 (3):521-565.

Ehrlich, I. 1975. “On the relation between education and crime.” In F.T. Juster (Ed.) Education, Income, and Human Behavior. McGraw-Hill Book Co., New York. 313-338.

Ellen, Ingrid Gould; Johanna Lacoe; and Claudia Sharygin. 2011. “Do Foreclosures Cause Crime?” National Institute of Justice Crime Mapping Conference, Miami, FL.

Engel, Justin. 2010. “Saginaw on Cost-Cutting Restructure Plan: ‘It Can’t be Bloodless’.” Mlive.com <http://www.mlive.com/news/saginaw/index.ssf/2010/01/saginaw_on_cost-cutting_restru.html>

Fagan, Jeffrey and Garth Davies. 2000. “Street Stops and Broken Windows: Terry, Race and Disorder in New York City.” *Fordham Urban Law Journal* Vol. 28:457.

Fleisher, Belton M. 1963. “The effect of unemployment on juvenile delinquency.” *Journal of Political Economy* 71 (6):543-555.

Fleisher, Belton M. 1966a. “The effect of income on delinquency.” *American Economic Review* 56:118-137.

Fleisher, Belton M. 1966b. *The economics of delinquency* Quadrangle: Chicago.

Glaeser, Edward, and Joseph Gyourko. 2005. “Urban Decline and Durable Housing.” *Journal of Political Economy* 113 (2):345-375.

Goodstein, Ryan, and Yan L. Lee. 2010. “Do foreclosures increase crime?” FDIC Center for Financial Research Working Paper No. 2010-05.

Griswold, Nigel. 2006. "The Impacts of Tax-Foreclosed Properties and Land Bank Programs on Residential Housing Values in Flint, Michigan." Michigan State University Master’s Thesis.

Harcourt, B. and Ludwig, J. 2006. “Broken Windows: New Evidence from New York City and a Five-City Social Experiment.” *The University of Chicago Law Review* 73:271-318.
Harris, Paul E. Lish. 2011. “A Closer Look at Home Foreclosures and Crime: Examining the Criminal Consequences of Home Foreclosures on Houston Neighborhoods.” The College of Criminology and Criminal Justice of Florida State University Doctoral Thesis.

Hartley, Daniel A. 2010. “Blowing it up and Knocking it Down: The Effect of Demolishing High Concentration Public Housing on Crime.” Federal Reserve Bank of Cleveland Working Paper 10-22.

Immergluck, Dan, and Geoff Smith. 2006. "The External Costs of Foreclosure: The Impact of Single-Family Mortgage Foreclosures on Property Values." *Housing Policy Debate* Fannie Mae Foundation. 17 (1):57-79.
Jacob, Brian A. 2004. “Public Housing, Housing Vouchers, and Student Achievement: Evidence from Public Housing Demolitions in Chicago.” *American Economic* Review 94 (1):233-258.

Jones, Roderick W. and William Alex Pridemore. 2012. “The Foreclosure Crisis and Crime: Is Housing-Mortgage Stress Associated with Violent and Property Crime in U.S. Metropolitan Areas?” *Social Science Quarterly* 93 (3):671.

Kelling, George L and James Q. Wilson. 1982. “Broken Windows.” The Atlantic Online http://www.theatlantic.com/magazine/archive/1982/03/broken-windows/304465/

Kirk, David S. and Derek S. Hyra. 2011. “Home Foreclosures and Community Crime: Causal or Spurious Association?” *Social Science Quarterly*. 93 (3):648-670.

Madensen, Tamara D., Timothy C. Hart, and Terance D. Miethe. 2011. “The Impact of Foreclosures on Neighborhood Crime in Nevada, 2006-09.” University of Las Vegas Center for the Analysis of Crime Statistics.

Pandit, Caaminee. 2011. “The Impact of Foreclosure on Crime in American Cities”. *Sociological Insight* 3:82-109.

Park, Robert E. and Ernest W. Burgess. 1925. *The city* Chicago, IL: University of Chicago Press.

Repetto, Thomas. 1974. “Crime prevention and the displacement phenomenon.” *Crime and Delinquency* 22 (2):166-177.

Schilling, Joseph, and Jonathan Logan. 2008. “Greening the Rust Belt: A Green Infrastructure Model for Right Sizing America’s Shrinking Cities.” Journal of the American Planning Association. 74 (4):451-466.

Shaw, C.R. and H.D. McCay. 1932. “Social factors in juvenile delinquency.” In Report of the Causes of Crime, Vol. 1. National Commission on Law Observance and Enforcement. Washington, DC: Government Printing Office.

Shaw, C.R. and H.D. McCay. 1942, 1969 revised. “Juvenile Delinquency and Urban Areas: a Study of Rates of Delinquency in Relation to Differential Characteristics of Local Communities in American Cities.” Chicago, IL: University of Chicago Press.
Sousa, William H. and George L. Kelling. 2002. “Policing Does Matter.” *The Manhattan Institute City Journal Winter*.
Spelman, William. 1993. “Abandoned Buildings: Magnets for Crime?” Journal of Criminal Justice 21 (5):481-495.

Sperling’s Best Places. 2009. “Best Places to Live in Saginaw, Michigan.” Web. <http://www.bestplaces.net/city/Saginaw-Michigan.aspx>

Stucky, Thomas D., John R. Ottensmann, and Seth B. Payton. 2012. “The Effect of Foreclosures on Crime in Indianapolis, 2003-2008”. *Social Science Quarterly* 93 (3):602-624.

Teasdale, Brent, Lynn M Clark, and Joshua C. Hinkle. 2012. “Subprime lending, crime, and neighborhood disorganization: Beyond internal dynamics.” *American Journal of Criminal Justice* 37 (2):163-178.

United States Department of Housing and Urban Development. 2011. “Neighborhood Stabilization Program Snapshot Reports.” http://hudnsphelp.info/index.cfm?do=viewSnapshotHome#pagetop

Winkelmann, Rainer. 2008. Econometric Analysis of Count Data 5th ed. Berlin; Springer.

Winthrop, Bob and Rebecca Herr. 2009. “Determining the Co$t of Vacancies in Baltimore.” *Government Finance Review* 38 (5).

Wooldridge, J. 1999. “Distribution-Free Estimation of Some Nonlinear Panel Data Models.” *Journal of Econometrics* 90 (1):77-97.

Wooldridge, J. 2002. Econometric Analysis of Cross Section and Panel Data 2nd ed. Cambridge, MA: Massachusetts Institute of Technology Press.

CHAPTER 2: LATENT MOTIVATIONS FOR THE IMPLEMENTATION OF MUNICIPAL RESIDENCY REQUIREMENT LAWS

Abstract

Public employee residency requirement laws have been a common feature of the American urban policy toolkit since the early 20th century. Many cities still require city residency for their employees, and others are attempting to reinstate such requirements. Justifications for these laws range from public safety to public coffer arguments. However, little is known about the underlying factors that motivate cities to implement residency laws. In this paper, I estimate a parametric duration model with a proportional hazard function to examine these latent motivations. In order to perform this analysis, I construct a unique data set on municipal residency laws for the largest 100 cities in the United States from 1970 through 2007. I also create a set of fiscal stress indicators for each of these cities over time. Results indicate that cities implement residency laws for economic and racial equity reasons and remove them when the city improves in comparison to the metropolitan area as a whole. Fiscal stress and service provision quality do not appear to be major contributing factors.

Introduction

Cities throughout the United States cite different reasons for employing residency requirement laws, or laws that require public employees to reside in the city or county of their employment. Some cities argue that public safety will be of higher quality if police and firemen live in the community in which they patrol so that they are more sensitive to their constituents and are quicker to respond to emergencies. Some argue that residency requirements ensure that the tax dollars spent by cities to pay for public employee salaries are recirculated in that city rather than exported to a nearby suburb. Still others argue that residency requirements increase minority representation and make hiring more representative of the demographics of a city.

In this paper I explore the underlying factors that motivate cities to implement and remove residency requirement laws by estimating a hazard model that examines the factors that lead up to the implementation of a residency law. I construct a unique data set on municipal residency laws for 73 of the largest 100 cities from 1970 through 2006. To the best of my knowledge, this is the first panel data set of these laws.

To account for city and census tract boundary changes over time, I use Brown University's Longitudinal Tract Database which standardizes census data from 1970 through 2000 to 2010 census tract boundaries.[40] I then take the census tracts that are within the city boundaries in 2010 and compare these over time. Finally, I merge these decennial city demographics with annual fiscal data from the Census of Governments and create fiscal stress indicators, as well as annual crime data from the Federal Bureau of Investigation's Uniform Crime Reports. Although many justifications are suggested for the implementation of these laws, I hypothesize that fiscal stress is the true motivation.

[40] Logan, Xu, and Stults, 2012

Only one previous paper examines this question directly. Eisinger (1983) creates a non-random cross section of 74 cities in 1980 to determine the characteristics of cities that have residency requirements versus those that do not. He finds that the adoption of residency laws is associated with unemployment, fiscal hardship, population loss, Frost Belt location, and mayor-council government. He also finds that, on average, cities which rescinded residency laws generally displayed stable or high population growth and low unemployment. Eisinger argues that the true motivation for all residency requirement laws is to improve the overall population and demographics of a city and that they are not actually a tool of public personnel management but an attempt to combat local unemployment and encourage the spending of city salaries in the local economy.

In this paper, I provide the first panel estimates of why cities choose to implement residency requirement laws. Results indicate that residency requirement laws are more likely to be implemented by cities with higher rates of unemployment, lower income per capita, and a larger number of black people living in the city versus the metropolitan area as a whole. Residency laws are more likely to be removed by cities with a lower income per capita but a higher income per capita in comparison to the metropolitan area as a whole. This implies that cities implement residency laws for economic and racial equity reasons and remove them when the city improves in comparison to the metropolitan area as a whole. Fiscal stress and service provision quality do not appear to be major contributing factors.

Background

Types of Residency Requirements

There are several types of municipal residency requirement laws. The first type, and the one of primary focus in this paper, is a law that requires municipal employees to live within the city boundary itself. This requirement can come in the form of a durational residency requirement in which prospective employees must prove that they have lived in the community for a prescribed period of time in order to be eligible for employment. The requirement can also be a continuing residency requirement which demands only that governmental employees live in the community during the term of their employment. The former has been found to impinge on the fundamental right to travel, forcing a governmental unit to show a compelling interest to justify a durational requirement.[41] Some residency requirements have been negotiated by unions for specific departments. These union negotiated requirements are not recorded in municipal archives and are therefore not included in my data set. However, this lack of data will only bias my results towards zero because if union negotiated contracts are in effect as residency requirements are implemented and removed, the results of the change in policy will be reduced.

Residency laws can also require that municipal employees live within the county boundary, within the state boundary, or within a certain radius of a municipality. Some states have used private local hire laws in which preference is given to city residents, but these have been deemed unconstitutional in at least one case.[42]

Cities have also used incentives and disincentives to encourage municipal employees to live within city boundaries. New York City's charter requires nonresident city employees to

[41] Myers, 1986

[42] "Local hire laws: Alaska's futile attempts at preferential treatment", 1987

make payments in the form of contractually agreed-upon salary deductions similar to an income tax.[43] Detroit, Michigan has recently instituted a residency incentive program called Project 14 which is meant to encourage police officers who live outside of Detroit to move into the city. Under Project 14, 200 tax-foreclosed houses are being offered for as little as $1,000 down, plus up to $150,000 in grants for renovations along with a monthly housing payment of $500 to $1,000.[44]

Residency requirements may be targeted at specific departments or they may be comprehensive amongst all employees. When targeted at specific departments, the rationale is usually that they are meant to improve the quality of the service provided by this department. For instance, most targeted residency requirements are focused on public safety departments with the belief being that if the public safety personnel live within the city not only will they be able to respond to emergencies more quickly, but they will also know the neighborhoods better and will therefore be able to provide better safety to the citizens.[45]

Comprehensive residency requirements that demand that all public employees live in the city or county are most often instituted for economic or demographic reasons. It is difficult to argue that an accountant's services will be improved by living in the city. More often, the accountant is forced to live in the city in order to increase the city's tax base and to improve the demographics within the city.[46]

---

[43] Boies, 2005/2006
[44] Nichols, 2011
[45] Duncan, 2005
[46] Duncan, 2005

Each of these types of residency requirements and incentive programs will have an impact on the makeup of the city in which it is instituted. In this paper I focus mainly on comprehensive city boundary residency requirements but also examine some aspects of other types of residency requirement laws.

Residency requirement laws were common in the early 1900s. However, these laws were gradually removed from most cities between 1920 and 1960 mainly based on the argument that such laws were a barrier to hiring the most qualified candidates.[47] There was a brief revival of the laws during the Great Depression, but by the late 1960s only Philadelphia, Buffalo, Milwaukee, and a few other big cities retained their residency requirements for police officers.[48] They became fashionable again in the 1970s, and by 1980 nearly two thirds of all cities over 250,000 in population had residency requirements.[49]

In 1976, the constitutionality of residency requirement laws was questioned in *McCarthy v. Philadelphia*. The Supreme Court upheld the Philadelphia residency requirement, and many cities soon followed with residency requirements of their own. Many cities that had dormant residency requirement laws began to enforce them after this court decision. Some states decided to ban residency requirements at the state level. California did so in 1974, for example, Michigan in 1999, and Ohio in 2009. Still, many cities throughout this time period reinstituted residency requirements and they remain a topic of debate to this day. In Michigan legislation will be introduced in September to remove the ban on residency requirements so that Detroit can

[47] Reed, 1941; International City Management Association, 1976
[48] Fogelson, 1977
[49] Eisinger, 1983

re-instate its requirement.[50] Despite the lack of evidence that these laws work, they are still a topic of discussion and discontent in many cities throughout the United States.

Rationales for Residency Requirement Laws

Justifications for municipal residency requirement laws fall into four main categories. The first is what is termed the "public coffer theory." This theory posits that residency requirement laws reduce local unemployment, benefit the local economy and increase the tax base.[51] The theory is based on the notion that salaries paid to civil servants should recirculate within the public and private economy of the city that pays those salaries.[52] Studies investigating the economic effects of residency laws on municipal labor markets have been contradictory. Hirsch and Rufalo (1986) find no effect of residency requirements on labor supply or demand, but do find effects on compensation because they alter the bargaining environment with unions. Gonzales, Mehay, and Duffy-Deno (1991) find that police employment is approximately 10 percent higher in cities with residency requirements which they suspect to be either due to the increase in the number of resident voters who are city employees and who are more likely to support spending increases, or due to an increase in productivity caused by the residency requirement. O'Brien (1997) finds that residency laws do not affect compensation and employment for either police or firefighters. Mehay and Seiden (1986) find that the gain in efficiency caused by residency requirements appears to have been lost due to the public employees' high demand for public services. They also argue that even if such laws do

[50] Angel, 2011
[51] Myers, 1986
[52] Eisinger, 1983

reduce per unit cost, the main benefit appears to accrue to the local bureaucracy in the form of greater output, expenditures, and agency size, and not to employees in the form of higher wages or to city residents in the form of tax relief.

The second type of rationale for residency requirements relates to the efficiency and quality of service provision. Many policy makers have argued that requiring municipal employees to live within the city boundaries or some other radius will improve their ability to provide services through increased knowledge of the community, 24 hour protection by public safety personnel[53], the fostering of employee concerns about the affairs of their city[54], the reduction of absenteeism and tardiness, and greater social symmetry between social servants and their constituents[55]. Smith (1980) provides empirical evidence of a positive relationship between the efficiency of a police department and the percentage of officers living in the community they serve. Gonzales, Mehay, and Duffy-Deno (1991) find that the actual crime rate tends to be below the predicted rate in cities that have residency requirements. However, many policy makers argue that residency requirements restrict the pool of the workforce which decreases the quality of applicants.[56] Residency requirements have also been opposed based on the freedom of employees to live where they please. This is often at the center of court cases against the laws.

Third, some cities have justified residency requirement laws based on minority representation issues. In Milwaukee, for example, the firefighters union is arguing for the preservation of their residency requirements partly in order to encourage the hiring of more black

[53] Smith, 1980
[54] City of Cincinnati, 1977
[55] Ector Versus City of Torrance, 1973
[56] Eisinger, 1983

and female firefighters.[57] Residency requirements are argued here to make municipal hiring representative of the demographics of a city. Stein (1986) studies the representativeness of local governments and finds that the presence of a residency requirement can offset the negative impact on representative hiring caused by civil service commissions, namely a 3% change.

Finally, the main argument which encompasses all of the previous arguments is that residency requirements improve the overall population and demographics of a city. Eisinger (1983) argues that this is the true motivation for all residency requirement laws and that they are not actually a tool of public personnel management but an attempt to combat local unemployment and encourage the spending of city salaries in the local economy. He finds that the adoption of residency laws is associated with unemployment, fiscal hardship, population loss, Frost Belt location, and mayor-council government. He also finds that, on average, cities which rescinded residency laws generally displayed stable or high population growth and low unemployment. The population size and demographics of a city may also benefit as a secondary effect from the intermediary effects discussed in the previous arguments. For instance, the intermediary effects from the public coffer theory (in which the city has a larger tax base which may imply an increased quantity and quality of services provided) may lead to a secondary effect of an increased number of people moving to the city. The efficiency argument, similarly, implies that there will be an intermediary effect of an increased quality of service provision such as decreased crime rates, which may also lead to more people moving to the city. Finally, the minority representation argument may lead to increased equality and decreased social unrest which may also make the city a more attractive place to live.

[57] Laasby, 2011

Data

To the best of my knowledge, no comprehensive historical data on residency requirements exists. To collect this data, I first searched historical city databases for the 100 largest cities in the United States to find city codes and charters that identify the years in which comprehensive residency requirements were implemented and removed as well as the details of each of these laws. However, most cities do not have electronic historical data available so I then combined this search with a search of law articles and historical newspapers. This also only produced data on the largest cities. To complete the data set, I then requested information from the missing cities individually through a series of phone calls to the city archivists. I attempted to collect data on residency requirements dating back to 1900, but many cities did not begin recording this data until the 1970's. Some cities were able to provide documentation to back up their facts; others simply provided the information based on memory. Out of the 100 cities from which I attempted to collect data, I was able to collect the history of residency requirement laws from 1970 to 2012 for 73 cities.

Table 2.1 lists the number of cities in my data set that had a residency requirement law on the decennial years. Figure 2.1 shows the number of residency laws that were implemented and removed each year. The negative bars represent removals of the law and the positive bars represent implementations of the law. Overall, 42 cities implemented various types of residency laws and 23 cities removed such laws. Table 2.2 summarizes the residency requirement data.

To account for city and census tract boundary changes over time, I use Brown University's Longitudinal Tract Database (LTDB) which standardizes census data from 1970 through 2000 to 2010 census tract boundaries. The LTDB provides estimates using 2010 boundaries for a standard set of variables from 1970 through 2000 for both full count variables

such as population, age, and race, as well as sample count variables based on the one-in-six samples from the decennial census and the American Community survey such as income and employment. I take the census tracts that are within the city boundaries in 2010 and compare these over time.

I only observe the demographic variables in my data set for the denial years. There is no perfect way to deal with the missing data for the years in between the census years. Interpolation and change variables both introduce endogeneity since these methods use future values to predict current values. I therefore choose to estimate the model using decennial demographic characteristics repeated each year until the next decennial year.

I then merge the residency law data and the decennial city demographics with annual fiscal data from the Census of Governments from 1970 through 2006. These fiscal data allow me to create fiscal stress indicators that measure how stressed the city is in each year as shown in Table 2.2. I also use fiscal stress measures from the decennial census such as per capita income and the rate of unemployment.

I define fiscal health using the definition proposed in Groves and Valente (2003). I then define fiscal stress as a deficiency in any of these measures. Fiscal health is defined as a government's ability to maintain solvency in four measures: cash, budgetary, long run, and service level. Cash solvency refers to a local government's liquidity and effective cash management, as well as its ability to pay current liabilities. Budgetary solvency refers to the ability of the government to generate sufficient revenues to fund its current or desired service levels. Long run solvency refers to the impact of existing long term obligations on future resources. Service level solvency refers to the ability of the government to provide and sustain a

service level that citizens require and desire. Fiscal stress is therefore defined as a deficiency in any of these measures.

Because no cash measure is included in the Census of Governments data, I can only measure cash solvency using a modified debt service ratio. The typical debt service ratio includes total interest on debt and principal payments. However, I only have information on total interest on debt from the Census of Governments data. I therefore measure the debt service as the interest on debt compared to total revenues.

I measure budgetary solvency using an operating deficit ratio which measures the operating surplus or deficit in comparison to total revenues. A deficit in one year does not necessarily indicate fiscal stress. A government may have unusually large expenditures in the current year but have planned for such events by saving money in previous years. Credit rating agencies are generally concerned when there is a budget deficit in two consecutive years. Because of this, I include an indicator for a budget deficit in two consecutive years or an abnormally large deficit in one year.

I also measure budgetary solvency using an intergovernmental revenues ratio, which measures how dependent the city is on intergovernmental transfers, and a property tax ratio, which measures the tax base of the city. Property tax is the main revenue source for most cities and therefore is a good indication of the health of a city's budget.

I measure service level solvency using crime rates from the Federal Bureau of Investigation's Uniform Crime Reports. The only crimes that are available for all years are what are known as the crime index offenses: murder, forcible rape, robbery, aggravated assault, burglary, larceny-theft, and motor vehicle theft. I sum all of these crimes into one overall crime level for each city/year

Unfortunately, the Census of Governments data does not provide any measures of long term solvency. However, these should be captured in the demographic variables that are included as controls.

Finally, I merge these data with crime data from the FBI's Uniform Crime Reports on offenses known and clearances by arrest for each year from 1970 through 2006. The only crimes that are available for all years are what are known as the crime index offenses: murder, forcible rape, robbery, aggravated assault, burglary, larceny-theft, and motor vehicle theft. I sum all of these crimes into one overall crime level for each city/year, and then also examine murder and vehicle theft separately. I examine murder on its own because of its seriousness, and I examine vehicle theft on its own because it is thought to be very closely correlated with the actual number of offenses because victims are likely to report vehicle thefts for insurance purposes. Murders are also likely to be reported.

There is a large literature pointing to inaccuracies in this FBI data. Namely, many city/years are endogenously missing because the data is self-reported. Some cities choose not to report their crime rates in years where they had a particularly high level of crime. Maltz (1999) advocates for the use of longitudinal rather than cross-section imputation techniques to correct for this missing data rather than the often used technique of using contemporaneous data from similar jurisdictions to impute the missing values. I, therefor, use longitudinal imputation techniques to interpolate the missing values in my data set. Descriptive statistics for each of the variables used in the regressions can be found in Table 2.4.

Econometric Model

To estimate the impact of different covariates on the decision to implement residency requirements, I estimate a parametric survival model of the time elapsed until implementation of a residency requirement. I then estimate a second model of the time elapsed from implementation until removal of a residency requirement in order to examine the motivations for removal of the laws.

Following Wooldridge, 2010, I model the probability of implementing or removing a residency requirement as a function of fiscal stress within the city, population, state bans on residency requirements, income differentials between the city and the MSA, race differentials between the city and the suburbs. To measure fiscal stress, I include per capita income, the rate of unemployment, the operating deficit ratio, the intergovernmental transfer ratio, the debt ratio, and the property tax revenue ratio as defined in the previous section.

I model the probability that a city will implement a residency requirement law using a proportional hazard model of the following form

$$\lambda(t;\mathbf{x}) = \kappa(\mathbf{x})\lambda_o(t) \qquad (1)$$

I then specify the baseline hazard, $\lambda_o(t)$, using a Weibull distribution because it allows for the duration to have positive or negative dependence as long as it is monotonically increasing or decreasing. The hazard then takes the form

$$\lambda(t;\mathbf{x}) = \exp(\mathbf{x}\beta)\alpha t^{\alpha-1} \qquad (2)$$

To account for time varying covariates, I let $\mathbf{x}(t)$ denote the vector of regressors at time *t*. For $t \geq 0$, let $\mathbf{X}(t)$, $t \geq 0$, denote the covariate path up through time *t*. Following Lancaster (1990, Chapter 2), I define the conditional hazard function at time *t* by

$$\lambda[t; \mathbf{X}(t)] = \lim_{h \downarrow 0} \frac{P[t \leq T < t + h \mid T \geq t, \mathbf{X}(t - h)]}{h} \quad (3)$$

This hazard function allows me to approximate the probability of implementing a residency requirement law within a short interval, conditional on having not implemented one up to the starting time of the interval.

I then let *T* denote the time until implementation of a residency requirement law. Because I do not observe the entire duration for all cities in my data set (i.e., some cities already had or still had residency requirements in 1970 and 2010, the beginning and end of my data), some data points are censored. To account for censoring, I let C_m be a binary censoring indicator equal to unity if the duration is censored in interval *m*, and zero otherwise. Because my sample includes all 100 largest US cities regardless of their initial or end state, this censoring is not systematic and therefore does not cause selection bias. This duration is therefore independent of censoring conditional on the covariates, an assumption of the model.

From this assumption, I can compute the probability that a city will implement (or remove) a residency requirement law as follows:

$$P(y_m = 1 \mid y_{m-1} = 0, \mathbf{x}, c_m = 0) =$$
$$P(a_{m-1} \leq T \lhd a_m \mid T \geq a_{m-1}, \mathbf{x})$$

$$= 1 - \exp\left[- \int_{a_{m-1}}^{a_m} \lambda(s; \mathbf{x}, \theta) ds \right] \equiv 1 - \alpha_m(\mathbf{x}, \theta) \tag{4}$$

for m=1,2,…,M, where

$$\alpha_m(\mathbf{x}, \theta) = \exp\left[- \int_{a_{m-1}}^{a_m} \lambda(s; \mathbf{x}, \theta) ds \right]$$

where the data is split into M + 1 intervals,

$[0, a_1), [a_1, a_2), ..., [a_{M-1}, a_M), [a_M, \infty)$, and y_m is a binary indicator equal to unity if the duration ends in the mth interval and zero otherwise.

From this, I construct a log likelihood for a single spell duration model with time-varying covariates with censoring is as follows:[58]

$$\sum_{h=1}^{m_i - 1} \log[\alpha_h(x_{im}, \theta)] + d_i \log[1 - \alpha_{m_i}(x_{im}, \theta)] \tag{5}$$

where d_i = 1 if duration i is uncensored.

[58] Wooldridge, 2002 p 711

I choose not to include unobserved heterogeneity in my model because it is difficult to relax the strict exogeneity assumption with time-varying covariates and unobserved heterogeneity included. Also, with single-spell data, I cannot allow general correlation between the unobserved heterogeneity and the covariates.[59]

Results

Residency Law Implementation

Table 2.5 reports the results of what variables affect the implementation of residency requirement laws. Results show that, conditional on the other covariates, cities with higher rates of unemployment, lower income per capita, and a larger number of black people living in the city versus the metropolitan area as a whole are more likely to implement residency requirement laws. Specifically, a city with a 1 percentage point higher unemployment rate, all else held equal, is 21.09% more likely to implement a residency requirement law. A city with 1% higher per capita income is 1.40% less likely to implement a residency requirement law. A city with a 1 percentage point larger number of black people living in the city versus the metropolitan area as a whole is 1.66% more likely to implement a residency requirement law. This follows the reasoning that some cities implement residency laws for reasons relating to racial equity in hiring; if more black people live in the city, the city may implement a residency law to ensure that hiring is representative of the racial distribution within the city.

Unemployment and income per capita are indicators of fiscal health within the city. However, the variables that more specifically relate to city finances are not statistically significant, such as the operating deficit ratio and the debt ratio. This may indicate that

[59] Wooldridge, 2002 p 713

economic indicators for the city are more correlated with residency law implementation than are the finances of the city government. This follows the theory that residency laws are implemented as a tool for helping the overall economy rather than simply the financial statements.

Residency Law Removal

Cities that remove residency laws are hypothesized to do so when their economy and fiscal health are improving. I therefore also examine the variables that cause cities to remove their residency laws. However, many cities in my data set removed their residency laws by force when a state ban was implemented. Therefore, identification of the removal of residency laws rests on the few cities who removed their laws by choice. Out of the 42 cities that had a residency requirement during my time period, 13 removed them because of state imposed bans. I removed these cities from this analysis because they are perfectly predicted by the ban.

Results, as presented in Table 2.6, show that, conditional on the other covariates, cities with a 1% higher income per capita are 3.094% less likely to remove their residency law. In addition, cities with a 1% higher income per capita in the city compared to the MSA as a whole are 4.647% more likely to remove their residency law. This implies that cities do not necessarily remove their residency laws when the demographics of the city improve, but when the demographics of the city compared to the MSA improve.

Conclusion

Residency requirement laws are more likely to be implemented by cities that have higher rates of unemployment, lower per capita income, and a larger proportion of black people living

in the city compared to the metropolitan area as a whole. These results imply that residency laws are implemented to better the economic health of a city – the "public coffer theory" -- and to make hiring representative of the racial mix of the city – the minority representation theory. It does not appear that population levels, public service provision, or municipal fiscal health are primary reasons for implementing residency requirement laws.

Residency laws are removed by cities with a lower income per capita but a higher income per capita in comparison to the metropolitan area as a whole. This implies that cities remove residency laws when the city improves in comparison to the metropolitan area as a whole.

Residency requirement laws have been a common feature in the American urban landscape since the early 20th century. Many cities still enforce residency for their employees, and some cities are in discussion to reinstate such laws. This paper indicates the underlying reasons why cities choose to implement such laws and lay the framework for the analysis of residency laws.

APPENDIX

APPENDIX

Tables and Figures

Figure 2.1: Changes to Residency Requirements, 1970 to 2006

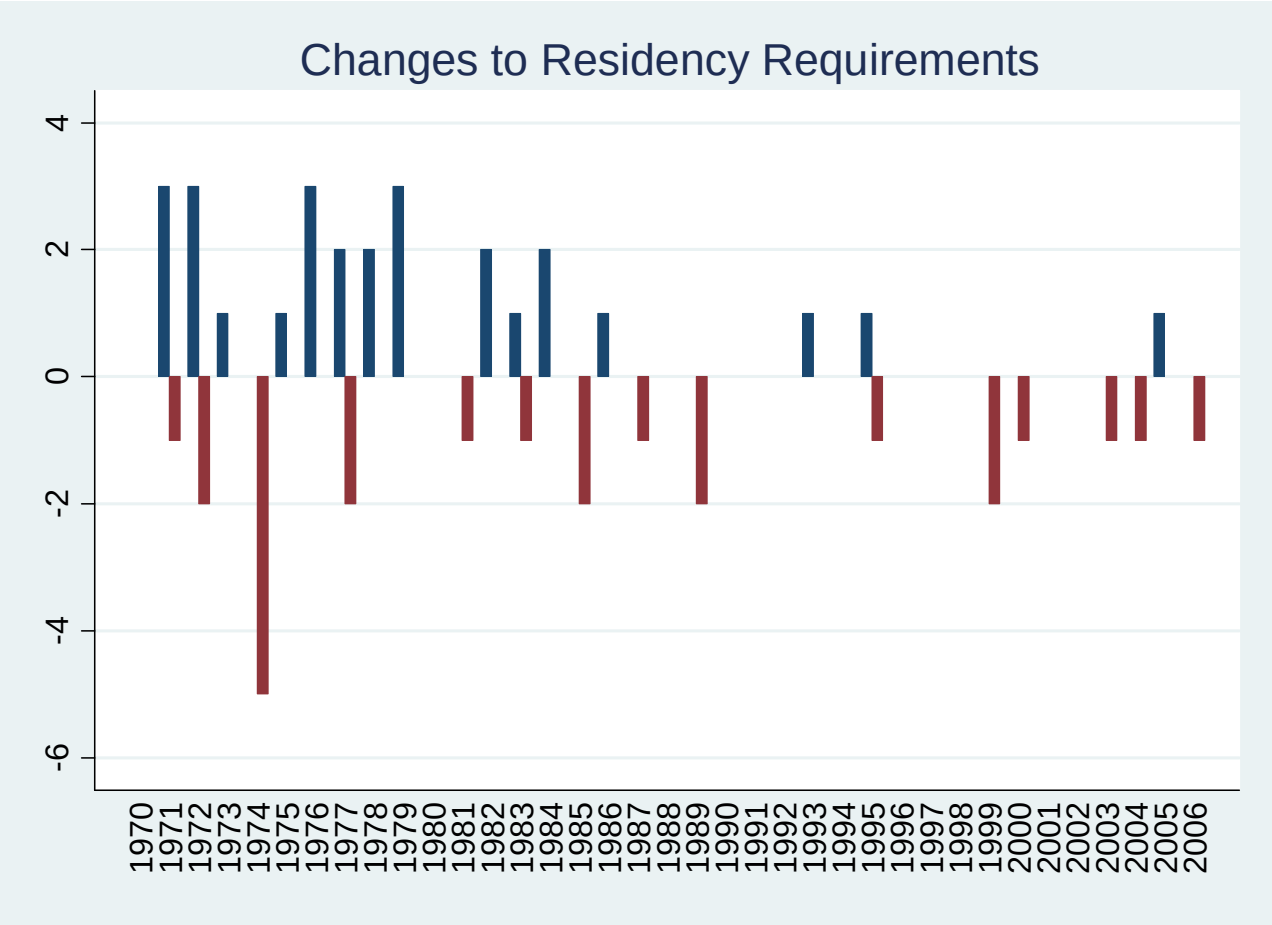


Table 2.1: Number of Cities with Residency Requirements from 1970 through 2010

Year	Number of Cities with a Residency Requirement Law
1970	15
1980	23
1990	22
2000	20
2010	17

Table 2.2: Residency Requirement Law Data

Residency Requirement Law	
Number of Cities Surveyed	100
Number of Responses	73
Number of Cities that had a Residency Law Between 1970 and 2006	42
Number of Cities that Removed a Residency Law Between 1970 and 2006	23
Number of Cities that Removed a Residency Law Between 1970 and 2006 because of a State Ban	13

Table 2.3: Fiscal Stress Ratios[60]

Large Operating Deficit Ratio	= ((Expenditures - Revenues)/ Revenues)>.05
Operating Deficit Ratio for Two Consecutive Years	= (Expenditures - Revenues)/ Revenues>0
Intergovernmental Revenue Ratio	= Intergovernmental Revenues/ Revenues
Debt Ratio	= Debt Interest/ Revenues
Property Tax Per Capita	= Property Tax Revenue/ Population

[60] Each of these variables is measured in 2010 dollars

Table 2.4: Descriptive Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Residency Requirement	2,701	0.29	0.45	0.00	1.00
Population	2,701	594,982	987,746	16,478	8,008,278
Crime	2,701	45,640	68,666	49	682,063
% Unemployed	2,701	6.5%	2.7%	2.1%	19.7%
Income Per Capita	2,701	$8,669.12	$6,369.27	$235.91	$32,163.32
Operating Deficit > 5%	2,701	0.54	0.50	0.00	1.00
Operating Deficit Two Consecutive Years	2,650	0.32	0.47	0.00	1.00
Intergovernmental Revenue Ratio	2,701	0.24	0.14	0.00	0.91
Debt Ratio	2,701	0.07	0.04	0.00	0.28
Property Tax Per Capita	2,701	$397.54	$362.26	$1.28	$2,514.23
# Black in City Divided by # Black in MSA	2,701	0.61	0.34	0.00	1.00
Income Per Capita in City Divided by Income Per Capita in MSA	2,701	1.05	0.27	0.15	2.23
Number of People Above Age 60	2,701	90,577	165,035	887	1,373,093
State Ban on Residency Requirements	2,701	0.22	0.42	0.00	1.00

Table 2.5: Variables that Influence Residency Requirement Implementation using Repeated Demographics

	Residency Requirement Law
Log of Population	-0.910
	(1.125)
Log of Crime	0.055
	(0.187)
% Unemployed	21.090*
	(12.490)
Log of Income Per Capita	-1.391***
	(0.300)
Operating Deficit Ratio > 5%	-0.268
	(0.493)
Operating Deficit Two Consecutive Years	-0.886
	(0.573)
Intergovernmental Revenue Ratio	1.158
	(2.014)
Debt Ratio	5.230
	(5.182)
Log of Property Tax Per Capita	0.471
	(0.387)
# Black in City divide by # Black MSA	1.663*
	(0.980)
Income per Capita in City divided by Income per Capita in MSA	0.596
	(0.688)
Log of Number of People above Age 60	1.283
	(1.075)
State Ban on Residency Requirements	-1.551
	(1.364)
Observations	1,383

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of the 100 largest cities in the United States from 1970-2007.

Table 2.6: Variables that Influence Residency Requirement Removal using Repeated Demographics

	Residency Requirement Law
Log of Population Repeated	-1.183
	(0.854)
Log of Crime	0.285
	(0.347)
% Unemployed Repeated	-25.580
	(15.970)
Log of Income Per Capita Repeated	-3.094***
	(0.416)
Operating Deficit Ratio > 5%	0.262
	(0.175)
Operating Deficit Two Consecutive Years	-0.074
	(0.262)
Intergovernmental Revenue Ratio	1.645
	(1.240)
Debt Ratio	4.149
	(4.079)
Log of Property Tax Per Capita	0.040
	(0.437)
# Black in City divide by # Black MSA Repeated	-0.720
	(1.522)
Income per Capita in City divided by Income per Capita in MSA Repeated	4.647**
	(2.021)
Log of Number of People above Age 60 Repeated	0.858
	(0.805)
Observations	792

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of the 100 largest cities in the United States from 1970-2007.

BIBLIOGRAPHY

BIBLIOGRAPHY

Angel, Cecil. "Detroiters tell legislators how ban on residency laws is hurting city." *Detroit Free Press* (Aug. 18, 2011). http://www.freep.com/article/20110819/NEWS01/108190413/Detroiters-tell-legislators-how-ban-residency-laws-hurting-city

Boies, Christopher G. "Fleming v. Giuliani." New York Law School Law Review. (2005/2006) 50:4 pp. 1003-1014.

City of Cincinnati. (1977). Ordinance of 29 June 1977.

Duncan, Brian. "Using Municipal Residency Requirements to Disguise Public Policy." *Public Finance Review.* 33: 84. (2005)

"Ector v. City of Torrance." (1973) 514 P 2nd 433.

Eisinger, Peter K. "Municipal Residency Requirements and the Local Economy." *Social Science Quarterly*. 64:1 (March 1983) p. 85

Fogelson, Robert. "Big City Police." Cambridge, Ma. Harvard University Press. (1977)

Gonzalez, Rodolfo A., Stephen L. Mehay, Kevin Duffy-Deno. "Municipal Residency Laws: Effects on Police Employment, Compensation, and Productivity." *Journal of Labor Research* (Fall 1991) 13:4

Groves, S. M., and M. G. Valente. (2003) "Evaluating financial condition: A handbook for local government." Washington, DC: International City/County Management Association.

Hirsch, Werner Z., Anthony M. Rufolo. "Economic Effects of Residency Laws on Municipal Police." Discussion paper #279. (January, 1983).

Hirsch, Werner Z., Anthony M. Rufolo. "Residence Laws and Unionization in Municipal Labor Markets: The Case of Firefighters." *Journal of Labor Research* (Winter, 1986) 7:1

International City Management Association. "Personnel Practices in the Municipal Police Service: 1976." *Urban Data Service Report* (Dec., 1976) pp. 2-14

Laasby, Gitte. "Firefighter group backs keeping residency rule." Journal Sentinel Online. (2011) http://www.jsonline.com/news/milwaukee/119739199.html

"Local hire laws: Alaska's futile attempts at preferential treatment." *Alaska Law Review* (Dec., 1987) pp. 359-386.

Logan, John R., Zengwang Xu, and Brian Stults. 2012. “Interpolating US Decennial Census Tract Data from as Early as 1970 to 2010: A Longitudinal Tract Database” *Professional Geographer*, forthcoming.

Maltz, M. (1999). *Bridging gaps in police crime data*. Washington, DC: U.S. Bureau of Justice Statistics.

Mehay, Stephen L., Kenneth P. Seiden. “Municipal Residency Laws and Local Public Budgets.” *Public Choice* (1986) 48:1 pp.27-35

Myers, Ross S. “The constitutionality of continuing residency requirements for local government employees: a second look.” *California Western Law Review* (Fall 1986) 23 p 24-41

Nichols, Darren A. “Bank of America donating houses to entice Detroit cops to live in city.” Detroit News (2011) http://detnews.com/article/20110323/METRO01/103230415/Bank-of-America-donating-houses-to-entice-Detroit-cops-to-live-in-city#ixzz1Jz84pw1v

O’Brien, Kevin M. “Do Municipal Residency Laws Affect Labour Market Outcomes?” *Urban Studies* (1997) 34:11 pp. 1759-1769

Reed, Thomas. “Municipal Management.” New York: McGraw Hill. (1941)

Smith, Dennis C. “Police Attitudes and Performance: The Impact of Residency.” *Urban Affairs Review* (1980) 15:317

Stein, Lana. “Representative Local Government: Minorities in the Municipal Work Force.” *The Journal of Politics* (Aug., 1986) 48:3 pp. 694-713

Wooldridge, Jeffrey. 2002. Econometric Analysis of Cross Section and Panel Data. Cambridge, MA: MIT Press.

CHAPTER 3: MUNICIPAL RESIDENCY REQUIREMENT LAWS AND THEIR IMPACT ON CITIES

Abstract

Over the past century, many U.S. cities have experimented with residency laws that require municipal employees to reside in the city or county of their employment. Despite the prevalence and extensive history of these laws, little is known about their impact on city outcomes. In this paper, I provide the first estimates of the impact of municipal residency requirements on the quality of service provision, municipal fiscal health, and the size and composition of cities. My identification strategy exploits the timing of municipal law changes in static and dynamic models. In order to perform the analysis, I construct an original data set on municipal residency laws for 73 of the largest 100 cities from 1970 through 2006. I then combine these data with information on city demographics from the Decennial Census, fiscal data from the Census of Governments, and crime data from the Federal Bureau of Investigation. Results provide little evidence that residency laws have an effect on fiscal health or city demographics when they are implemented, but they are correlated with a temporary increase then long term decrease in crime. The removal of a residency law is not associated with any significant change in fiscal health, city demographics, or crime.

Introduction

Over the past century, many U.S. cities have experimented with residency requirement laws that require municipal employees to reside in the city or county of their employment. New York City, for example, implemented such a law in 1937, removed it in 1962, and then re-implemented in 1986. An event in 2011 in which a police officer made a racially charged remark on the internet has rekindled debate over the law with proponents arguing that forcing police officers to live in the city would increase their awareness and sensitivity toward their constituents.[61] Detroit, Michigan, implemented a residency requirement in 1913 and was forced to remove it in 1999 when the state banned such laws. Policy makers in Detroit are calling for the removal of the ban so that the city can once again enforce the law.[62] The governor of Wisconsin recently implemented a state ban on residency requirements has incited criticism from large cities and small villages throughout the state.[63]

Despite the prevalence and extensive history of these requirements, little is known about their impact on municipal service provision, fiscal health, or city demographics. In this paper I provide the first estimates of the impact of comprehensive municipal residency requirements on city outcomes. My identification strategy exploits the timing of municipal law changes. I also examine the static and dynamic effects of the laws. In order to perform the analysis, I construct an original data set on municipal residency laws for 73 of the largest 100 cities from 1970 through 2006. To the best of my knowledge, this is the first panel data set of these laws that includes more than two years.

---

[61] New York News, 2011

[62] Angel, 2011

[63] Stein and Walker, 2013

To account for city and census tract boundary changes over time, I use Brown University's Longitudinal Tract Database which standardizes census data from 1970 through 2000 to 2010 census tract boundaries. I then take the census tracts that are within the city boundaries in 2010 and compare these back over time. Finally, I merge these decennial city demographics with annual fiscal data from the Census of Governments and annual crime data from the Federal Bureau of Investigation's Uniform Crime Reports.

Previous research has shown, using a difference in difference of two years, that a residency requirement can increase a city's middle class population but only if it is accompanied by a municipal wage premium.[64] This suggests that in cities where the government is not able to offer a wage premium over its more wealthy suburbs like in Detroit, Michigan, a residency requirement will not have its desired demographic effect. In addition, it is likely that when implemented, a residency requirement will encourage the most qualified employees (the ones who can easily find a different job) to switch jobs rather than to move. The less qualified employees will be more likely to move to keep their current job because their outside prospects are fewer.

However, even if these requirements do encourage only the less qualified employees to move into the city, these employees may improve in quality because of their increased connection with the community and quicker response times for emergencies. They also may have a greater incentive to improve the quality of their work since it will improve the neighborhood in which they and their families live. In addition, these new residents will increase

[64] Duncan, 2005

the property tax base for the city at least in the short run, even if expenditures also increase due to their new incentive to vote for spending increases.[65]

My results show that the implementation of a residency law may be correlated with a slight increase in crime two years after the law is implemented, and then a reduction in crime six or more years after implementation. The removal of a law is correlated with an increase in crime, but these results disappear once a time trend is included, indicating that this is likely caused by city trends rather than by the law itself. Residency laws may be correlated with a reduction in intergovernmental revenues four and six or more years after it is implemented. This may imply a reduction in fiscal stress but it is unclear using that indicator alone. Finally, residency laws and their removal do not appear to be correlated with any changes in demographics or population.

Background

Types of Residency Requirements

There are several types of municipal residency requirement laws. The first type, and the one of primary focus in this paper, is a law that requires municipal employees to live within the city boundary itself. This requirement can come in the form of a durational residency requirement in which prospective employees must prove that they have lived in the community for a prescribed period of time in order to be eligible for employment. The requirement can also be a continuing residency requirement which demands only that governmental employees live in the community during the term of their employment. The former has been found to impinge on the fundamental right to travel, forcing a governmental unit to show a compelling interest to

[65] Gonzales, Mehay, and Duffy-Deno, 1991

justify a durational requirement.[66] Some residency requirements have been negotiated by unions for specific departments. These union negotiated requirements are not recorded in municipal archives and are therefore not included in my data set. However, this lack of data will only bias my results towards zero because if union negotiated contract are in effect as residency requirements are implemented and removed, the results of the change in policy will be reduced. Some of my city-year observations that I assign to the control group are actually in the treated group because of the union negotiated contracts. This makes the difference in coutcomes for the treatment and control groups less large because some of the control group is actually being treated.

Residency laws can also require that municipal employees live within the county boundary, within the state boundary, or within a certain radius of a municipality. Some states have used private local hire laws in which preference is given to city residents, but these have been deemed unconstitutional in at least one case.[67]

Cities have also used incentives and disincentives to encourage municipal employees to live within city boundaries. New York City's charter requires nonresident city employees to make payments in the form of contractually agreed-upon salary deductions similar to an income tax.[68] Detroit, Michigan has recently instituted a residency incentive program called Project 14 which is meant to encourage police officers who live outside of Detroit to move into the city. Under Project 14, 200 tax-foreclosed houses are being offered for as little as $1,000 down, plus

[66] Myers, 1986
[67] "Local hire laws: Alaska's futile attempts at preferential treatment", 1987
[68] Boies, 2005/2006

up to $150,000 in grants for renovations along with a monthly housing payment of $500 to $1,000.[69]

Residency requirements may be targeted at specific departments or they may be comprehensive amongst all employees. When targeted at specific departments, the rationale is usually that they are meant to improve the quality of the service provided by this department. For instance, most targeted residency requirements are focused on public safety departments, with the belief that if the public safety personnel live within the city, they be able to respond to emergencies more quickly and they will know the neighborhoods better and will therefore be able to provide better safety to the citizens.[70]

Comprehensive residency requirements that demand that all public employees live in the city or county are most often instituted for economic or demographic reasons. It is difficult to argue that an accountant's services will be improved by living in the city. More often, the accountant is forced to live in the city in order to increase the city tax base and improve the demographics in the city.[71]

Each of these types of residency requirements and incentive programs will have an impact on the makeup of the city in which it is instituted. In this paper I focus on comprehensive city boundary residency requirements.

Residency requirement laws were common in the early 1900s. However, these laws were gradually removed from most cities between 1920 and 1960 mainly based on the argument that

[69] Nichols, 2011
[70] Duncan, 2005
[71] Duncan, 2005

such laws were a barrier to hiring the most qualified candidates.[72] There was a brief revival of the laws during the Great Depression, but by the late 1960s only Philadelphia, Buffalo, Milwaukee, and a few other big cities retained their residency requirements for police officers.[73] They became fashionable again in the 1970s and by 1980 nearly two thirds of all cities with populations over 250,000 had residency requirements.[74]

In 1976, the constitutionality of residency requirement laws was questioned in *McCarthy v. Philadelphia*. The Supreme Court upheld the Philadelphia residency requirement, and many cities soon followed with residency requirements of their own. Many cities that had dormant residency requirement laws began to enforce them after this court decision. Some states chose to ban residency requirements at the state level. California did so in 1974, Michigan in 1999, and Ohio later in 2009. Still, many cities throughout this time period reinstituted residency requirements and they remain a topic of debate to this day. In Michigan, legislation has been introduced to remove the ban on residency requirements so that Detroit can re-instate its city residency requirement.[75] Despite the lack of evidence that these laws work, they are still a topic of discussion and discontent in many cities throughout the United States.

Pros and Cons of Residency Requirement Laws

Advocates of residency requirements claim four main benefits. The first relates to what has been labeled the "public coffer theory." This theory posits that residency requirement laws

[72] Reed, 1941; International City Management Association, 1976
[73] Fogelson, 1977
[74] Eisinger, 1983
[75] Angel, 2011

reduce local unemployment, benefit the local economy and increase the tax base.[76] The theory is based on the notion that salaries paid to civil servants should recirculate within the public and private economy of the city that pays those salaries.[77]

Opponents argue that residency requirements do not actually benefit the public coffer, but instead encourage an increase in expenditures and government size due to the employees now living and voting in the municipality. This residency, they argue, increases the demand for public services and payrolls, and thus expenditures.

Studies investigating the economic effects of residency laws on municipal labor markets have been contradictory. Hirsch and Rufalo (1986) find no effect of residency requirements on labor supply or demand, but do find effects on compensation because they alter the bargaining environment with unions. Gonzales, Mehay, and Duffy-Deno (1991) find that police employment is approximately 10 percent higher in cities with residency requirements which they suspect to be either due to the increase in the number of resident voters who are city employees and who are more likely to support spending increases, or due to a decrease in productivity caused by the residency requirement. Mehay and Seiden (1986) find that the gain in efficiency caused by residency requirements appears to have been lost due to the public employees' high demand for public services. They also argue that even if such laws do reduce per unit cost, the main benefit appears to accrue to the local bureaucracy in the form of greater output, expenditures, and agency size, and not to employees in the form of higher wages or to city residents in the form of tax relief. O'Brien (1997) finds that residency laws do not affect compensation and employment for either police or firefighters. I examine the public coffer

[76] Myers, 1986
[77] Eisinger, 1983

theory by estimating the effect of residency requirements on municipal fiscal health ratios, population, income per capita, and unemployment.

The second type of rationale for residency requirements relates to the efficiency and quality of service provision. Some policy makers argue that requiring municipal employees to live within the city boundaries or some other radius will improve their ability to provide services through increased knowledge of the community, 24 hour protection by public safety personnel[78], the fostering of employee concerns about the affairs of their city[79], the reduction of absenteeism and tardiness, and greater social symmetry between social servants and their constituents[80].

Opponents argue, however, that requiring residency will restrict the pool of the workforce which decreases the quality of applicants.[81] The quality of public services may decline because the best employees will have better outside options for employment and may choose to find a new job rather than relocate into the city.

Two papers have examined this relationship. Smith (1980) finds a positive relationship between the efficiency of a police department and the percentage of officers living in the community they serve, though this is likely correlation rather than causation. Gonzales, Mehay, and Duffy-Deno (1991) find that the actual crime rate tends to be below the predicted rate in cities that have residency requirements. I examine the service provision theory by estimating the effect of residency requirements on crime.

The third rationale for residency requirement laws is based on minority representation issues. In Milwaukee, for example, the firefighters union is arguing for the preservation of their

[78] Smith, 1980
[79] City of Cincinnati, 1977
[80] Ector Versus City of Torrance, 1973
[81] Eisinger, 1983

residency requirements partly in order to encourage the hiring of more black and female firefighters.[82] Residency requirements are argued to make municipal hiring representative of the demographics of a city. Stein (1986) studies the representativeness of local governments and finds that the presence of a residency requirement can offset the negative impact on representative hiring caused by civil service commissions, namely a 3% change. However, residency requirements may actually be discriminatory if the majority of minorities lives outside of the city. The National Association for the Advancement of Colored People (NAACP) has been opposed to residency requirements in cities where this is the case, but a proponent of them where minorities live within the cities rather than outside of them. I do not have data on the racial makeup of municipal employees and can therefore not test this theory directly. See Plerhoples (2013) for an analysis of whether racial distribution is a significant factor for implementation of residency laws.

The final rationale for residency requirements is that they improve the overall population and demographics of a city. Eisinger (1983) argues that this is the true motivation for all residency requirement laws and that they are not actually a tool of public personnel management but an attempt to combat local unemployment and encourage the spending of city salaries in the local economy. He finds that the adoption of residency laws is associated with unemployment, fiscal hardship, population loss, Frost Belt location, and mayor-council government. He also finds that, on average, cities that rescind residency laws generally displayed stable or high population growth and low unemployment. I examine this effect using panel techniques and an instrumental variables approach to remove the endogeneity caused by the choice of when to implement residency laws.

[82] Laasby, 2011

No study has analyzed the overall impact of the laws on population or demographic changes. This paper fills this gap in the literature by providing the first causal estimates of how residency requirements affect the overall population and demographics within a city.

Empirical Methodology

To estimate the causal impact of residency requirements on the population and demographic characteristics of cities, I employ a linear fixed effects panel data model. I analyze a variety of dependent variables relating to crime, fiscal health, and demographics which will be discussed in detail in the next section.

The basic model is as follows:

$$y_{it} = \beta_0 + \beta_1 \mathbf{x}_{it-1} + \theta_i + \gamma_t + u_{it} \qquad (1)$$

where y_{it} are the different population and demographic characteristics (estimated separately); $\mathbf{x}_{it}$ is a vector of independent variables including whether or not the city has a residency requirement; θ_i are individual fixed effects; γ_t are time fixed effects; and u_{it} is the error term.

In order to examine and account for time trends, I estimate both a contemporaneous model with residency laws specified as a zero one indicator variable and a dynamic model with the residency laws specified as a flow variable defined as the first difference of the indicator variable. In the dynamic model, I include six leads and five lags of residency requirements in

order to examine the varying effects over time and to check for spikes in independent variables before a residency law is implemented. I also include a variable that equals the one if the city ever had a residency law six or more years before year *t*. This allows me to examine the effect of a residency law in period *t* compared to seven years before the law was implemented, as well as six years before and after implementation of the law.

Data

To the best of my knowledge, no comprehensive historical data on residency requirements exists. To collect these data, I first searched through available city databases for the 100 largest cities in the United States to find historical city codes and charters to identify the years in which comprehensive residency requirements were implemented and removed as well as details of each law. However, most cities do not have electronic historical data available, so I then combined this search with a search of law articles and historical newspapers. This also only produced data on the largest cities. To complete the data set, I then requested information from the missing cities individually through of series of emails and phone calls. I attempted to collect data on residency requirements back to 1900, but many cities do not have these data on record before the 1970s. Some cities were able to provide documentation to back up their facts; others simply provided the information based on memory. Out of the 100 cities from which I attempted to collect data, I was able to collect the history of residency requirement laws back to 1970 for 73 cities. Table 3.1 shows the number of cities in my sample that reported having a residency requirement law in each decade.

To account for city and census tract boundary changes over time, I use Brown University's Longitudinal Tract Database (LTDB) which standardizes census data from 1970

through 2000 to 2010 census tract boundaries. The LTDB provides estimates using 2010 boundaries for a standard set of variables from 1970 through 2000 for both full count variables such as population, age, and race, as well as sample count variables based on the one-in-six samples from the decennial census and the American Community survey such as income and employment.

I then take the census tracts that are within the city boundaries in 2010 and compare these over time. Finally, I merge these decennial city demographics with yearly fiscal data from the Census of Governments and yearly crime data from the Federal Bureau of Investigation's Uniform Crime Reports. Descriptive statistics for all of the variables can be found in Table 3.2. Table 3.3 show the number of cities that responded to the inquiry and the number that had and removed a law during the study period.

Results

The Effect of Residency Requirement Laws on Crime

In order to examine the effect of residency requirement laws on crime, I merged data from the FBI's Uniform Crime Reports on offenses known and clearances by arrest for each year from 1970 through 2006. The only crime data that are available for all years are what are known as the crime index offenses: murder, forcible rape, robbery, aggravated assault, burglary, larceny-theft, and motor vehicle theft. I sum all of these crimes into one overall crime level for each city/year.

There is a large literature pointing to inaccuracies in this FBI data.[83] Namely, many city/years are endogenously missing because the data are self-reported. Some cities choose not

[83] Maltz, 1999; Lynch and Jarvis, 2008; Akiyama and Propheter, 2005 to name a few

to report their crime rates in years where they had a particularly high level of crime. Maltz (1999) advocates for the use of longitudinal rather than cross-section imputation techniques to correct for this missing data rather than the often used technique of using contemporaneous data from similar jurisdictions to impute the missing values. I, therefore, use longitudinal imputation techniques to interpolate the missing values in my data set.

Table 3.4 presents the results of the contemporaneous effect of residency requirement laws on crime. The results suggest that residency laws are not correlated with changes in the sum of the seven Crime Index offenses. To check whether time trends are confounding these results, I run the same model including individual city by year variables. These results can be seen in Table 3.5. Residency laws remain insignificant in this model.

Table 3.6 presents the results of the dynamic model which includes six lags and six leads of residency laws. Residency laws appear to be correlated with a slight increase in crime two years after the law is in place (by 4.2%), but then a decline in crime six or more years after the law is implemented (by 14.5%). This may imply that there are long run decreases in crime after a residency law is implemented. These numbers are large, but so are the confidence intervals which implies that there is an effect but we do not know whether it is large or small. The coefficients are imprecisely measured so we should not read too much into the point estimates.

In these regressions I control for lagged population, lagged income per capita, the lagged percentage of residents who are between the ages of 18 and 25 (because this is considered to be the age range that contains the highest crime rates), lagged race variables (specifically percent black and percent not black nor white because these are the only breakdowns available in 1970), the percent of residents with a high school degree, and the percent of vacant housing units in the city. All of the covariates that are significant are of the expected sign. I chose not to include

police expenditures as a covariate due to concerns about feedback and endogeneity; police expenditures are likely to increase when crime increases. This violates the strict exogeneity requirement for a fixed effects model.

I also examined the effect of removing a residency law on crime in these cities. Table 3.7 presents these results. I remove all city/years from the data set that are before a residency law is implemented so that I am only comparing the post residency removal period to the residency period. The sum of the Crime Index offenses is significant and negative, indicating that the removal of a residency requirement law is associated with a reduction in crime index crimes by 11.6%. However, once I include a time trend in the model, the coefficient becomes insignificant. Therefore, it is unclear whether the reduction in crime seen after the removal of a residency law is caused by the removal of the law or because the cities that remove the laws have decreasing crime around the time of the removal. In the dynamic model results, shown in Table 3.9, the six lags and leads of the removal of a residency law are not statistically significant, implying that there is no significant change in crime after the removal of such a law.

The Effect of Residency Requirement Laws on Municipal Fiscal Health

Residency laws are hypothesized to affect the fiscal health of a city. As city employees move into the city, they have an incentive to vote for increased expenditures. I measure fiscal stress using the five fiscal health ratios shown in Table 3.10.

I define fiscal health using the definition proposed in Groves and Valente (2003). I then define fiscal stress as a deficiency in any of these measures. Fiscal health is defined as a government's ability to maintain solvency in four measures: cash, budgetary, long run, and service level. Cash solvency refers to a local government's liquidity and effective cash

management, as well as its ability to pay current liabilities. Budgetary solvency refers to the ability of the government to generate sufficient revenues to fund its current or desired service levels. Long run solvency refers to the impact of existing long term obligations on future resources. Service level solvency refers to the ability of the government to provide and sustain a service level that citizens require and desire. Fiscal stress is therefore defined as a deficiency in any of these measures.

Because no cash measure is included in the Census of Governments data, I can only measure cash solvency using a modified debt service ratio. The typical debt service ratio includes total interest on debt and principal payments. However, I only have information on total interest on debt from the Census of Governments. I therefore measure the debt service as the interest on debt compared to total revenues.

I measure budgetary solvency using an operating deficit ratio which measures the operating surplus or deficit in comparison to total revenues. A deficit in one year does not necessarily indicate fiscal stress. A government may have unusually large expenditures in the current year but have planned for such events by saving money in previous years. Credit rating agencies are generally concerned when there is a budget deficit in two consecutive years. Because of this, I include an indicator for a budget deficit in two consecutive years or an abnormally large deficit in one year.

I also measure budgetary solvency using an intergovernmental revenues ratio, which measures how dependent the city is on intergovernmental transfers, and a property tax ratio, which measures the tax base of the city. Property tax is the main revenue source for most cities and therefore is a good indication of the health of the city's budget.

Unfortunately, the Census of Governments data do not provide any measures of long term solvency. However, these are partially captured in the demographic variables that are estimated in the next section. The crime results in the previous section represent service level solvency analysis.

I find no evidence that lagged residency laws affect the fiscal health of a city as measured by these five fiscal stress ratios as can be seen in Tables 3.11 and again in Table 3.12 where I include a time trend. I control for population, income per capita, race, percent of residents above the age of 60, education, and number of vacant housing units. Income per capita could be either positive or negative because as income increases, often both expenditures and revenues do as well. There is also no clear prediction for the effect of race. We might expect that as a city ages, the government will be more fiscally stressed because there are fewer people working and providing tax income, and more people getting pensions and retiree health care services. However, I find this not to be true, controlling for the other covariates. Education and income per capita are closely correlated and may therefore be hard to interpret.

Table 3.13 presents the results of the dynamic model. Here we see a slight dip in the probability of having a large operating deficit ratio in the years before a residency law is implemented, then a decrease again four, five, and six or more years after the law is implemented. However, we see a slight increase in the probability of having an operating deficit ratio two years in a row two and one years before the law is implemented, then an increased chance four, five, and six or more years after the law is implemented. As discussed earlier, having one or the other of these variables be positive does no alone indicate stress. Therefore, since these results are of the opposite sign, they do not indicate a clear implication for the effect of residency laws on fiscal stress. We also see a slight increase in the intergovernmental revenue

ratios of cities the year before the implementation of a residency law, and then a reduction four and six or more years after. This may imply an improvement in the fiscal health of a city after a residency law is implemented.

I examine the effect of the removal of a residency requirement law on municipal fiscal health in Tables 3.14 and 3.15. Both models, with and without time trends, show no significant effect of the removal of residency laws on the fiscal health of cities.

In the dynamic model, presented in Table 3.16, we see an elevated level of property taxes both before and after the residency law is removed. This may be due to overall trends in the cities that remove the laws.

The Effect of Residency Requirement Laws on Demographics

Due to the nature of the census data, estimating the effect of residency requirement laws on demographics is more difficult. Using the city boundaries in 2010 back over time, I can only obtain demographic variables for the decennial years. I therefore estimate the model with the data collapsed to the decennial years. This model is not ideal because it is so aggregated, so results should be only used as a first glance at the effect of the laws on demographics.

Tables 3.17 and 3.18 present the results of the effect of residency requirement laws on population, unemployment, and per capita income with data collapsed to the decennial years both with and without including a time trend. I find no evidence that residency laws have a statistically significant effect on population, unemployment, or income per capita.

In these models, I control for covariates that are thought to affect each dependent variable. Glaeser, Scheinkman, and Shleifer (1995) find that population growth moves with income growth, and that they are both positively related to schooling, negatively related to

unemployment, and negatively related to employment in manufacturing. I chose to include only income per capita and not unemployment in my model since they are difficult to interpret when both are included. I also include other covariates thought to affect population like crime, property taxes, race, the share of the population that is elderly, and housing status. Similar covariates are present in my percent unemployed and income per capita models. Sachs (2003) argues that income per capita is caused by geography which is captured in city fixed effects in my model, and quality of institutions which is captured in fiscal stress variables in my model.

Tables 3.19 and 3.20 present the results of the effect of the removal of residency requirements on these variables collapsed to the decennial years, with and without instruments. Once again, we see no statistically significant effect of the removal of residency laws on population, percent unemployed, and income per capita.

Because the data is so aggregated in the previous analyses, I further explore the effect of residency laws on population by estimating the model using yearly population estimates from the Census of Governments. This model is also not ideal because these population estimates do not control for city boundary changes. However, the two sets of results combined provide some idea of the effect.

Tables 3.21 and 3.22 present the results of this analysis with and without a time trend. I find no statistically significant effect again. When I examine this model using dynamics, I again find no significant correlation as can be seen in Table 3.23.

Tables 3.24 and 3.25 use these same data to examine the effect of the removal of residency laws on population. I once again find no significant effect with or without a time trend, or dynamics. Removing a residency law does not appear to cause mass exodus from the city as some policymakers fear.

Conclusion

My results show that the implementation of a residency law may be correlated with a slight increase in crime two years after the law is implemented, and then a reduction in crime six or more years after implementation. The removal of a law is correlated with an increase in crime, but these results disappear once a time trend is included, indicating that this is likely caused by city trends rather than by the law itself.

Residency laws may be correlated with a reduction in intergovernmental revenues four and six or more years after it is implemented. This may imply a reduction in fiscal stress but it is unclear using that indicator alone. Finally, residency laws and their removal do not appear to be correlated with any changes in demographics or population.

These results provide no evidence that residency requirement laws remedy cities of high crime rates, fiscal stress, or demographic ills. In addition, removing a residency laws do not appear to cause mass exodus from the city.

APPENDIX

APPENDIX

Tables and Figures

Table 3.1: Number of Cities in dataset with Residency Requirements from 1970 to 2010

Year	Number of Cities with Residency Requirement Laws
1920	3
1930	5
1940	9
1950	9
1960	15
1970	15
1980	24
1990	23
2000	20
2010	17

Table 3.2: Descriptive Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Residency Requirement	73	0.589	0.495	0.000	1.000
Population	2701	607,026	989,563	16,478	8,107,637
Crime	2701	45,640	68,666	49	682,063
% Age 18 to 25	2701	0.131	4%	6%	38%
% > Age 60	2701	0.145	4%	3%	37%
% Black	2701	0.209	17%	0%	86%
% Other Races	2701	0.044	5%	0%	37%
% High School Degree	2701	0.568	14%	16%	90%
% College Degree	2701	0.209	9%	4%	58%
% Vacant Units	2701	0.074	3%	-1%	22%
% Owner Occupied Units	2701	0.534	10%	18%	83%
% Manufacturing Jobs	2701	0.151	7%	0%	42%
% Professional Jobs	2701	0.281	7%	10%	64%
Per Capita Expenditures	2701	$2.39	$1.78	$0.40	$16.78
Income Per Capita	2701	$10,984.00	$7,095.50	$235.91	$40,813.89
Property Tax Per Capita	2701	0	$0.36	$0.00	$2.51
Debt Ratio	2701	0.069	0.042	0.000	0.282
Operating Deficit Ratio > 5%	2701	0.544	0.498	0.000	1.000
Operating Deficit Ratio Two Consecutive years	2650	0.317	0.466	0.000	1.000
Intergovernmental Revenue Ratio	2694	0.236	0.141	0.000	0.913
% Unemployed	2701	7%	3%	2%	20%

Table 3.3: City Residency Laws

Residency Requirement Law	
Number of Cities Surveyed	100
Number of Responses	73
Number of Cities that had a Residency Law Between 1970 and 2006	42
Number of Cities that Removed a Residency Law Between 1970 and 2006	23
Number of Cities that Removed a Residency Law Between 1970 and 2006 because of a State Ban	13

Table 3.4: Mean Effect of Residency Requirement Laws on Crime

	(1) Crime
Lagged Residency Requirement Law	0.001
	(0.045)
Lagged Log of Population Interpolated	0.995***
	(0.159)
Lagged Log of Income Per Capita Interpolated	0.017
	(0.122)
Lagged % Age 18 to 25 Interpolated	0.417
	(0.836)
Lagged % Black Interpolated	0.636
	(0.384)
Lagged % Other Races Interpolated	-3.956***
	(0.708)
Lagged % High School Degree Interpolated	-0.798
	(0.657)
Lagged % Vacant Units Interpolated	0.394
	(0.886)
Observations	2,628
R-squared	0.617
Number of Cities	73

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of 73 of the 100 largest cities in the US (as of 2010) from 1970- 2006.

Table 3.5: Mean Effect of Residency Requirement Laws on Crime with Time Trend

	(1) Crime
Lagged Residency Requirement Law	-0.003
	(0.043)
Lagged Log of Population Interpolated	1.008***
	(0.308)
Lagged Log of Income Per Capita Interpolated	0.147
	(0.130)
Lagged % Age 18 to 25 Interpolated	-0.473
	(0.504)
Lagged % Black Interpolated	0.213
	(1.202)
Lagged % Other Races Interpolated	0.850
	(2.349)
Lagged % High School Degree Interpolated	0.132
	(0.966)
Lagged % Vacant Units Interpolated	0.368
	(0.937)
Observations	2,628
R-squared	0.778
Number of Cities	73

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of 73 of the 100 largest cities in the US (as of 2010) from 1970- 2006.

Table 3.6: Dynamic Effects of Residency Requirement Laws on Crime

	(1) Crime
Six years before Residency Law Implementation	0.026
	(0.032)
Five years before Residency Law Implementation	0.033
	(0.042)
Four years before Residency Law Implementation	0.024
	(0.045)
Three years before Residency Law Implementation	-0.006
	(0.055)
Two years before Residency Law Implementation	-0.018
	(0.072)
One year before Residency Law Implementation	-0.025
	(0.076)
Year of Residency Law Implementation	0.034
	(0.036)
One year after Residency Law Implementation	0.040
	(0.026)
Two years after Residency Law Implementation	0.042*
	(0.021)
Three years after Residency Law Implementation	Omitted
Four years after Residency Law Implementation	0.010
	(0.021)
Five years after Residency Law Implementation	-0.033
	(0.031)
Six or more years after Residency Law Implementation	-0.145***
	(0.048)
Lagged Log of Population Interpolated	1.328***
	(0.369)
Lagged Log of Income Per Capita Interpolated	-0.137
	(0.337)
Lagged % Age 18 to 25 Interpolated	1.930
	(1.430)
Lagged % Black Interpolated	-0.057
	(0.976)
Lagged % Other Races Interpolated	-9.993***
	(3.478)
Lagged % High School Degree Interpolated	-1.328
	(1.210)

Table 3.6 (cont'd)

	(1) Crime
Lagged % Vacant Units Interpolated	4.984**
	(1.878)
Observations	578
R-squared	0.477

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of 73 of the 100 largest cities in the US (as of 2010) from 1970- 2006.

Table 3.7: Mean Effect of the Removal of Residency Requirement Laws on Crime

	(1) Crime
Lagged Removal of Residency Requirement Law	-0.116*
	(0.060)
Lagged Log of Population Interpolated	0.833***
	(0.146)
Lagged Log of Income Per Capita Interpolated	0.07
	(0.116)
Lagged % Age 18 to 25 Interpolated	-0.139
	(0.803)
Lagged % Black Interpolated	0.381
	(0.388)
Lagged % Other Races Interpolated	-3.476***
	(0.558)
Lagged % High School Degree Interpolated	-1.136*
	(0.654)
Lagged % Vacant Units Interpolated	0.481
	(0.862)
Observations	2,392
R-squared	0.657
Number of Cities	73

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of 73 of the 100 largest cities in the US (as of 2010) from 1970- 2006.

Table 3.8: Mean Effect of the Removal of Residency Requirement Laws on Crime with Time Trend

	(1) Crime
Lagged Removal of Residency Requirement Law	-0.037
	(0.068)
Lagged Log of Population Interpolated	0.831**
	(0.328)
Lagged Log of Income Per Capita Interpolated	0.153
	(0.137)
Lagged % Age 18 to 25 Interpolated	-0.965*
	(0.492)
Lagged % Black Interpolated	0.122
	(1.315)
Lagged % Other Races Interpolated	0.909
	(2.055)
Lagged % High School Degree Interpolated	-0.1
	(1.034)
Lagged % Vacant Units Interpolated	0.689
	(0.959)
Observations	2,392
R-squared	0.809
Number of Cities	73

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of 73 of the 100 largest cities in the US (as of 2010) from 1970- 2006.

Table 3.9: Dynamic Effects of the Removal of Residency Requirement Laws on Crime

	(1) Crime
Six years before Residency Law Removal	-0.035
	(0.039)
Five years before Residency Law Removal	-0.034
	(0.050)
Four years before Residency Law Removal	-0.029
	(0.055)
Three years before Residency Law Removal	0.015
	(0.060)
Two years before Residency Law Removal	0.042
	(0.070)
One year before Residency Law Removal	0.042
	(0.065)
Year of Residency Law Removal	0.039
	(0.068)
One year after Residency Law Removal	0.032
	(0.068)
Two years after Residency Law Removal	0.024
	(0.062)
Three years after Residency Law Removal	0.010
	(0.064)
Four years after Residency Law Removal	-0.026
	(0.088)
Five years after Residency Law Removal	-0.005
	(0.062)
Six or more years after Residency Law Removal	-0.029
	(0.067)
Lagged Log of Population Interpolated	0.851***
	(0.168)
Lagged Log of Income Per Capita Interpolated	0.034
	(0.148)
Lagged % Age 18 to 25 Interpolated	-0.213
	(0.744)
Lagged % Black Interpolated	0.187
	(0.436)
Lagged % Other Races Interpolated	-3.814***
	(0.764)
Lagged % High School Degree Interpolated	-1.828**
	(0.786)

Table 3.9 (cont'd)

	(1) Crime
Lagged % Vacant Units Interpolated	2.554** (1.206)
Observations	1,785
R-squared	0.595
Number of Cities	72

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of 73 of the 100 largest cities in the US (as of 2010) from 1970- 2006.

Table 3.10: Fiscal Stress Ratios[84]

Ratio	**Calculation**
Large Operating Deficit Ratio	= ((Expenditures - Revenues)/ Revenues)>.05
Operating Deficit Ratio for Two Consecutive Years	= (Expenditures - Revenues)/ Revenues>0
Intergovernmental Revenue Ratio	= Intergovernmental Revenues/ Revenues
Debt Ratio	= Debt Interest/ Revenues
Property Tax Per Capita	= Property Tax Revenue/ Population

[84] Each of these variables is converted into 2010 dollars

Table 3.11: Mean Effect of Residency Requirement Laws on Municipal Fiscal Health

	(1)	(2)	(3)	(4)	(5)
	Operating Deficit Ratio > 5%	Operating Deficit Two Consecutive Years	Intergovern-mental Revenue Ratio	Debt Ratio	Log of Property Tax Per Capita
Lagged Residency Requirement Law	-0.020	0.025	-0.001	-0.001	0.001
	(0.037)	(0.047)	(0.010)	(0.003)	(0.048)
Lagged Log of Population Interpolated	0.003	0.094	0.006	-0.011	0.024
	(0.155)	(0.169)	(0.032)	(0.017)	(0.168)
Lagged Log of Income Per Capita Interpolated	0.257*	-0.360**	0.040	0.005	0.285*
	(0.133)	(0.136)	(0.029)	(0.017)	(0.153)
Lagged % Black Interpolated	0.210	-0.570	0.486***	-0.153***	-0.463
	(0.670)	(0.738)	(0.099)	(0.058)	(0.683)
Lagged % Other Races Interpolated	-1.711*	1.322	-0.001	0.035	-0.789
	(0.863)	(0.952)	(0.196)	(0.100)	(1.047)
Lagged % > Age 60 Interpolated	-0.398	0.297	-0.252	-0.271**	-0.167
	(0.983)	(1.040)	(0.222)	(0.117)	(1.046)
Lagged % High School Degree Interpolated	0.920	-1.131*	0.301***	-0.117*	-0.854
	(0.636)	(0.579)	(0.109)	(0.068)	(0.651)
Lagged % Vacant Units Interpolated	-0.541	0.823	0.326	0.138	1.027
	(1.132)	(1.226)	(0.231)	(0.094)	(0.919)
Observations	2,628	2,628	2,622	2,622	2,617
R-squared	0.084	0.081	0.411	0.246	0.241
Number of Cities	73	73	73	73	73

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of 73 of the 100 largest cities in the US (as of 2010) from 1970- 2006.

Table 3.12: Mean Effect of Residency Requirement Laws on Municipal Fiscal Health with Time Trend

	(1) Operating Deficit Ratio > 5%	(2) Operating Deficit Two Consecutive Years	(3) Intergovern-mental Revenue Ratio	(4) Debt Ratio	(5) Log of Property Tax Per Capita
Lagged Residency Requirement Law	-0.008	0.021	-0.001	0.002	0.007
	(0.047)	(0.052)	(0.009)	(0.004)	(0.039)
Lagged Log of Population Interpolated	-0.371	0.388	-0.075	0.062	0.852**
	(0.487)	(0.437)	(0.087)	(0.046)	(0.349)
Lagged Log of Income Per Capita Interpolated	0.157	-0.192	-0.004	-0.006	0.320***
	(0.226)	(0.245)	(0.032)	(0.019)	(0.120)
Lagged % Black Interpolated	1.211	-1.421	0.240	-0.114	-0.510
	(1.236)	(1.280)	(0.189)	(0.110)	(0.969)
Lagged % Other Races Interpolated	2.703	-3.497	0.049	0.527	2.848
	(2.819)	(3.048)	(0.504)	(0.369)	(3.235)
Lagged % > Age 60 Interpolated	-0.065	-0.435	-0.903	-0.122	2.841**
	(2.771)	(2.783)	(0.660)	(0.287)	(1.392)
Lagged % High School Degree Interpolated	-0.792	0.855	0.162	-0.145	-0.234
	(1.479)	(1.651)	(0.254)	(0.197)	(0.906)
Lagged % Vacant Units Interpolated	0.612	-0.362	0.217	0.307**	1.202
	(1.338)	(1.496)	(0.198)	(0.144)	(1.031)
Observations	2,628	2,628	2,622	2,622	2,617
R-squared	0.151	0.177	0.585	0.439	0.492
Number of Cities	73	73	73	73	73

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of 73 of the 100 largest cities in the US (as of 2010) from 1970- 2006.

Table 3.13: Dynamic Effects of Residency Requirement Laws on Municipal Fiscal Health

	(1) Operating Deficit Ratio > 5%	(2) Operating Deficit Two Consecutive Years	(3) Intergovern-mental Revenue Ratio	(4) Debt Ratio	(5) Log of Property Tax Per Capita
Six years before Residency Law Implementation	-0.081	0.064	0.008	-0.022	-0.073
	(0.145)	(0.124)	(0.012)	(0.014)	(0.050)
Five years before Residency Law Implementation	-0.336**	0.120	0.014	-0.017	-0.116
	(0.161)	(0.143)	(0.016)	(0.014)	(0.072)
Four years before Residency Law Implementation	-0.316*	0.079	0.002	-0.019	-0.099
	(0.157)	(0.146)	(0.021)	(0.016)	(0.084)
Three years before Residency Law Implementation	-0.167	0.124	0.026	-0.014	-0.047
	(0.180)	(0.132)	(0.022)	(0.016)	(0.061)
Two years before Residency Law Implementation	-0.418***	0.182*	0.034	-0.017	-0.083
	(0.115)	(0.102)	(0.023)	(0.014)	(0.065)
One year before Residency Law Implementation	-0.253*	0.189*	0.041*	-0.019	-0.081
	(0.142)	(0.105)	(0.023)	(0.016)	(0.072)
Year of Residency Law Implementation	-0.145	0.115	-0.015	0.000	0.061
	(0.151)	(0.109)	(0.016)	(0.005)	(0.077)
One year after Residency Law Implementation	-0.179	0.192	-0.013	-0.001	-0.021
	(0.187)	(0.139)	(0.014)	(0.005)	(0.033)
Two years after Residency Law Implementation	-0.033	0.039	-0.018	0.003	0.003
	(0.163)	(0.113)	(0.012)	(0.004)	(0.040)

Table 3.13 (cont'd)

	(1) Operating Deficit Ratio > 5%	(2) Operating Deficit Two Consecutive Years	(3) Intergovern-mental Revenue Ratio	(4) Debt Ratio	(5) Log of Property Tax Per Capita
Three years after Residency Law Implementation	Omitted	Omitted	Omitted	Omitted	Omitted
Four years after Residency Law Implementation	-0.319***	0.184**	-0.015*	0.000	-0.033
	(0.116)	(0.090)	(0.009)	(0.003)	(0.031)
Five years after Residency Law Implementation	-0.287*	0.282**	-0.001	0.001	-0.032
	(0.165)	(0.119)	(0.010)	(0.005)	(0.031)
Six or more years after Residency Law Implementation	-0.228*	0.246**	-0.034***	0.004	0.008
	(0.128)	(0.098)	(0.011)	(0.007)	(0.042)
Lagged Log of Population Interpolated	0.360	-0.753	-0.034	0.076	0.929**
	(0.615)	(0.906)	(0.121)	(0.081)	(0.358)
Lagged Log of Income Per Capita Interpolated	-0.436	0.252	0.236**	-0.056	-0.142
	(0.593)	(0.652)	(0.092)	(0.041)	(0.432)
Lagged % Black Interpolated	-1.588	1.118	0.539	-0.339**	-0.545
	(1.001)	(1.326)	(0.323)	(0.144)	(1.379)
Lagged % Other Races Interpolated	2.018	-1.815	3.214*	-0.246	-5.285
	(4.960)	(5.488)	(1.788)	(0.509)	(4.818)
Lagged % > Age 60 Interpolated	-0.828	0.268	-0.618	-0.448	2.511
	(4.266)	(5.256)	(1.162)	(0.497)	(3.861)

Table 3.13 (cont'd)

	(1) Operating Deficit Ratio > 5%	(2) Operating Deficit Two Consecutive Years	(3) Intergovern-mental Revenue Ratio	(4) Debt Ratio	(5) Log of Property Tax Per Capita
Lagged % High School Degree Interpolated	3.193	-2.284	1.017*	-0.003	-2.607
	(2.225)	(2.048)	(0.549)	(0.243)	(1.690)
Lagged % Vacant Units Interpolated	-4.791*	0.891	0.135	0.643**	2.510
	(2.822)	(3.028)	(0.860)	(0.276)	(3.744)
Observations	578	578	578	578	578
R-squared	0.146	0.125	0.447	0.359	0.113
Number of Cities	34	34	34	34	34

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of 73 of the 100 largest cities in the US (as of 2010) from 1970- 2006.

Table 3.14: Mean Effect of the Removal of Residency Requirement Laws on Municipal Fiscal Health

	(1) Operating Deficit Ratio > 5%	(2) Operating Deficit Two Consecutive Years	(3) Intergovern-mental Revenue Ratio	(4) Debt Ratio	(5) Log of Property Tax Per Capita
Lagged Removal of Residency Requirement Law	0.038	-0.018	-0.018	0.006	0.043
	(0.064)	(0.074)	(0.015)	(0.006)	(0.045)
Lagged Log of Population Interpolated	0.056	0.053	0.003	-0.006	0.082
	(0.177)	(0.183)	(0.037)	(0.019)	(0.161)
Lagged Log of Income Per Capita Interpolated	0.23	-0.330**	0.042	0.002	0.233
	(0.145)	(0.144)	(0.032)	(0.019)	(0.152)
Lagged % Black Interpolated	0.314	-0.77	0.524***	-0.154**	-0.326
	(0.723)	(0.782)	(0.103)	(0.064)	(0.694)
Lagged % Other Races Interpolated	-1.988**	1.558*	-0.034	0.061	-0.254
	(0.827)	(0.926)	(0.180)	(0.101)	(1.021)
Lagged % > Age 60 Interpolated	-0.321	0.006	-0.357*	-0.213*	0.029
	(1.078)	(1.143)	(0.211)	(0.114)	(1.190)
Lagged % High School Degree Interpolated	1.072	-1.192*	0.296**	-0.106	-1.098
	(0.683)	(0.621)	(0.118)	(0.071)	(0.685)
Lagged % Vacant Units Interpolated	-0.62	0.955	0.287	0.166*	0.951
	(1.175)	(1.249)	(0.224)	(0.096)	(0.892)
Observations	2,392	2,392	2,386	2,386	2,381
R-squared	0.086	0.082	0.395	0.227	0.254
Number of Cities	73	73	73	73	73

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of 73 of the 100 largest cities in the US (as of 2010) from 1970- 2006.

Table 3.15: Mean Effect of the Removal of Residency Requirement Laws on Municipal Fiscal Health with Time Trend

	(1) Operating Deficit Ratio > 5%	(2) Operating Deficit Two Consecutive Years	(3) Intergovern-mental Revenue Ratio	(4) Debt Ratio	(5) Log of Property Tax Per Capita
Lagged Removal of Residency Requirement Law	0.041	-0.024	-0.007	-0.007	-0.033
	(0.078)	(0.084)	(0.013)	(0.008)	(0.041)
Lagged Log of Population Interpolated	-0.272	0.394	-0.078	0.074	0.888**
	(0.566)	(0.499)	(0.090)	(0.048)	(0.383)
Lagged Log of Income Per Capita Interpolated	0.089	-0.103	-0.003	-0.010	0.298**
	(0.250)	(0.275)	(0.034)	(0.021)	(0.121)
Lagged % Black Interpolated	2.500*	-2.420*	0.290	-0.098	-1.111
	(1.472)	(1.362)	(0.252)	(0.145)	(0.956)
Lagged % Other Races Interpolated	3.688	-4.531	-0.334	0.281	2.685
	(2.833)	(3.004)	(0.509)	(0.327)	(3.307)
Lagged % > Age 60 Interpolated	-0.963	-0.172	-0.731	-0.057	2.556*
	(3.058)	(3.085)	(0.664)	(0.323)	(1.445)
Lagged % High School Degree Interpolated	-1.196	1.930	0.149	-0.159	-0.084
	(1.775)	(1.883)	(0.285)	(0.233)	(0.987)
Lagged % Vacant Units Interpolated	0.911	-0.739	0.191	0.328**	0.642
	(1.412)	(1.557)	(0.212)	(0.159)	(1.042)
Observations	2,392	2,392	2,386	2,386	2,381
R-squared	0.161	0.188	0.59	0.425	0.483
Number of Cities	73	73	73	73	73

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of 73 of the 100 largest cities in the US (as of 2010) from 1970- 2006.

Table 3.16: Dynamic Effect of the Removal of Residency Requirement Laws on Municipal Fiscal Health

	(1) Operating Deficit Ratio > 5%	(2) Operating Deficit Two Consecutive Years	(3) Intergovern-mental Revenue Ratio	(4) Debt Ratio	(5) Log of Property Tax Per Capita
Six years before Residency Law Removal	-0.028	-0.006	-0.019*	0.024*	0.085**
	(0.138)	(0.113)	(0.010)	(0.013)	(0.041)
Five years before Residency Law Removal	0.233	-0.046	-0.018	0.019	0.102**
	(0.143)	(0.130)	(0.013)	(0.014)	(0.046)
Four years before Residency Law Removal	0.207	-0.035	-0.005	0.022	0.090
	(0.147)	(0.141)	(0.017)	(0.015)	(0.066)
Three years before Residency Law Removal	0.129	-0.083	-0.030	0.013	0.117*
	(0.128)	(0.104)	(0.021)	(0.014)	(0.061)
Two years before Residency Law Removal	0.331***	-0.132	-0.014	0.018	0.122*
	(0.107)	(0.100)	(0.022)	(0.012)	(0.062)
One year before Residency Law Removal	0.108	-0.166*	-0.019	0.023*	0.120*
	(0.092)	(0.089)	(0.023)	(0.013)	(0.068)
Year of Residency Law Removal	0.076	0.050	-0.035*	0.021	0.192***
	(0.109)	(0.113)	(0.020)	(0.014)	(0.059)
One year after Residency Law Removal	0.181	0.012	-0.019	0.017	0.178***
	(0.109)	(0.106)	(0.021)	(0.015)	(0.063)
Two years after Residency Law Removal	0.123	-0.037	-0.024	0.021	0.167**
	(0.108)	(0.097)	(0.027)	(0.014)	(0.065)

Table 3.16 (cont'd)

	(1) Operating Deficit Ratio > 5%	(2) Operating Deficit Two Consecutive Years	(3) Intergovern-mental Revenue Ratio	(4) Debt Ratio	(5) Log of Property Tax Per Capita
Three years after Residency Law Removal	0.157	0.024	-0.021	0.016	0.163**
	(0.111)	(0.106)	(0.028)	(0.014)	(0.064)
Four years after Residency Law Removal	0.110	-0.080	-0.033	0.017	0.198***
	(0.123)	(0.111)	(0.026)	(0.013)	(0.071)
Five years after Residency Law Removal	0.140	0.017	-0.015	0.017	0.030
	(0.113)	(0.130)	(0.028)	(0.013)	(0.096)
Six or more years after Residency Law Removal	0.154	-0.110	-0.043	0.017	0.116
	(0.122)	(0.126)	(0.029)	(0.013)	(0.073)
Lagged Log of Population Interpolated	-0.009	0.110	0.010	0.008	0.196
	(0.238)	(0.247)	(0.051)	(0.024)	(0.183)
Lagged Log of Income Per Capita Interpolated	0.425**	-0.528**	0.051	0.003	0.174
	(0.207)	(0.224)	(0.044)	(0.023)	(0.181)
Lagged % Black Interpolated	0.816	-1.275	0.418***	-0.215**	-0.672
	(0.873)	(0.884)	(0.142)	(0.082)	(0.752)
Lagged % Other Races Interpolated	-2.146**	2.106*	-0.080	0.057	-1.083
	(1.049)	(1.163)	(0.250)	(0.126)	(1.305)
Lagged % > Age 60 Interpolated	-1.310	0.937	-0.373	-0.236	-0.348
	(2.212)	(2.632)	(0.337)	(0.161)	(1.431)

Table 3.16 (cont'd)

	(1) Operating Deficit Ratio > 5%	(2) Operating Deficit Two Consecutive Years	(3) Intergovern-mental Revenue Ratio	(4) Debt Ratio	(5) Log of Property Tax Per Capita
Lagged % High School Degree Interpolated	2.788**	-2.795**	0.282*	-0.071	-1.184
	(1.111)	(1.205)	(0.151)	(0.099)	(0.735)
Lagged % Vacant Units Interpolated	-1.822	2.366	0.252	0.303**	1.789
	(1.516)	(1.539)	(0.353)	(0.138)	(1.357)
Observations	1,785	1,785	1,785	1,785	1,785
R-squared	0.07	0.072	0.452	0.269	0.22
Number of Cities	72	72	72	72	72

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of 73 of the 100 largest cities in the US (as of 2010) from 1970- 2006.

Table 3.17: The Effect of Residency Requirement Laws on Municipal Demographics

Collapsed to Decennial Years

	(1) Log Population	(2) Percent Unemployed	(3) Income Per Capita
Lagged Residency Requirement Law	-0.007	0.003	-0.007
	(0.023)	(0.003)	(0.032)
Lagged Log of Crime	0.132***	0.006**	0.273***
	(0.032)	(0.002)	(0.065)
Lagged Log of Property Tax Per Capita	-0.021	-0.003	0.126*
	(0.032)	(0.004)	(0.072)
Lagged Debt Ratio	0.132	0.067**	0.076
	(0.198)	(0.030)	(0.436)
Lagged Per Capita Expenditures	-0.082**	0.002	-0.168*
	(0.041)	(0.006)	(0.095)
Lagged Log of Income Per Capita Interpolated	0.560***		
	(0.064)		
Lagged % Black Interpolated	-0.855***	0.078***	-1.304***
	(0.258)	(0.021)	(0.288)
Lagged % Other Races Interpolated	1.531***	-0.131**	2.450**
	(0.536)	(0.051)	(1.013)
Lagged % > Age 60 Interpolated	0.182	-0.010	5.769***
	(0.749)	(0.034)	(1.249)
Lagged % High School Degree Interpolated	-0.453	0.264***	-1.349
	(0.486)	(0.034)	(0.932)
Lagged % College Degree Interpolated	-1.025	0.071	2.681**
	(0.749)	(0.074)	(1.309)
Lagged % Manufacturing Jobs Interpolated	0.042	0.006	0.603
	(0.340)	(0.031)	(0.634)
Lagged % Professional Jobs Interpolated	-1.084*	0.150**	-1.229
	(0.586)	(0.071)	(1.388)
Lagged % Vacant Units Interpolated	-0.671*		
	(0.397)		

Table 3.17 (cont'd)

	(1) Log Population	(2) Percent Unemployed	(3) Income Per Capita
Lagged % Owner Occupied Units Interpolated	0.284		
	(0.314)		
Observations	291	291	291
R-squared	0.912	0.421	0.951
Number of Cities	73	73	73

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of 73 of the 100 largest cities in the US (as of 2010) from 1970, 1980, 1990, 2000, and 2006.

Table 3.18: The Effect of Residency Requirement Laws on Municipal Demographics

Collapsed to Decennial Years with Time Trend

	(1) Log Population	(2) Percent Unemployed	(3) Income Per Capita
Lagged Residency Requirement Law	-0.018	0.004	0.020
	(0.015)	(0.007)	(0.046)
Lagged Log of Crime	0.054**	0.015**	0.013
	(0.021)	(0.006)	(0.041)
Lagged Log of Property Tax Per Capita	0.004	-0.009	0.102
	(0.018)	(0.007)	(0.074)
Lagged Debt Ratio	0.052	0.106**	0.325
	(0.134)	(0.044)	(0.339)
Lagged Per Capita Expenditures	-0.051	0.009	-0.006
	(0.034)	(0.011)	(0.099)
Lagged Log of Income Per Capita Interpolated	0.125***		
	(0.041)		
Lagged % Black Interpolated	-1.159**	0.407***	-1.136
	(0.548)	(0.089)	(0.852)
Lagged % Other Races Interpolated	-0.020	0.221	-5.379**
	(0.760)	(0.330)	(2.168)
Lagged % > Age 60 Interpolated	-3.629***	0.347	-3.172
	(0.870)	(0.269)	(2.150)
Lagged % High School Degree Interpolated	-1.687***	0.134	-0.546
	(0.510)	(0.116)	(1.188)
Lagged % College Degree Interpolated	-1.898**	0.293	5.186***
	(0.796)	(0.218)	(1.757)
Lagged % Manufacturing Jobs Interpolated	0.381	0.203	-1.813
	(0.553)	(0.126)	(1.189)
Lagged % Professional Jobs Interpolated	0.762	0.103	-1.621
	(0.517)	(0.196)	(1.715)
Lagged % Vacant Units Interpolated	-0.482*		
	(0.289)		

Table 3.18: The Effect of Residency Requirement Laws on Municipal Demographics

Collapsed to Decennial Years with Time Trend

	(1)	(2)	(3)
	Log Population	Percent Unemployed	Income Per Capita
Lagged % Owner Occupied Units Interpolated	0.456		
	(0.326)		
Observations	291	291	291
R-squared	0.989	0.636	0.991
Number of Cities	73	73	73

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of 73 of the 100 largest cities in the US (as of 2010) from 1970, 1980, 1990, 2000, and 2006.

Table 3.19: The Effect of the Removal of Residency Requirement Laws on Municipal Demographics Collapsed to Decennial Years

	(1) Log Population	(2) Percent Unemployed	(3) Income Per Capita
Lagged Removal of Residency Requirement Law	0.014	-0.004	-0.027
	(0.033)	(0.005)	(0.049)
Lagged Log of Crime	0.124***	0.006**	0.276***
	(0.031)	(0.003)	(0.069)
Lagged Log of Property Tax Per Capita	-0.029	-0.003	0.145*
	(0.035)	(0.004)	(0.079)
Lagged Debt Ratio	0.157	0.069**	0.095
	(0.187)	(0.032)	(0.465)
Lagged Per Capita Expenditures	-0.087*	-0.001	-0.140
	(0.047)	(0.006)	(0.106)
Lagged Log of Income Per Capita Interpolated	0.565***		
	(0.066)		
Lagged % Black Interpolated	-0.926***	0.086***	-1.185***
	(0.283)	(0.023)	(0.326)
Lagged % Other Races Interpolated	1.469**	-0.135**	2.126*
	(0.555)	(0.057)	(1.083)
Lagged % > Age 60 Interpolated	0.481	-0.013	6.014***
	(0.777)	(0.037)	(1.316)
Lagged % High School Degree Interpolated	-0.621	0.273***	-1.152
	(0.498)	(0.039)	(0.918)
Lagged % College Degree Interpolated	-1.529*	0.101	2.946**
	(0.786)	(0.078)	(1.448)
Lagged % Manufacturing Jobs Interpolated	0.038	0.033	0.789
	(0.392)	(0.035)	(0.699)
Lagged % Professional Jobs Interpolated	-0.790	0.139*	-1.413
	(0.646)	(0.070)	(1.473)
Lagged % Vacant Units Interpolated	-0.598		
	(0.398)		

Table 3.19 (cont'd)

	(1) Log Population	(2) Percent Unemployed	(3) Income Per Capita
Lagged % Owner Occupied Units Interpolated	0.239		
	(0.322)		
Observations	278	278	278
R-squared	0.917	0.400	0.949
Number of Cities	73	73	73

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of 73 of the 100 largest cities in the US (as of 2010) from 1970, 1980, 1990, 2000, and 2006.

Table 3.20: The Effect of the Removal of Residency Requirement Laws on Municipal Demographics Collapsed to Decennial Years with Time Trend

	(1) Log Population	(2) Percent Unemployed	(3) Income Per Capita
Lagged Removal of Residency Requirement Law	0.021	-0.005	-0.007
	(0.021)	(0.009)	(0.049)
Lagged Log of Crime	0.063***	0.011*	0.007
	(0.023)	(0.006)	(0.039)
Lagged Log of Property Tax Per Capita	-0.006	-0.010	0.144**
	(0.022)	(0.007)	(0.070)
Lagged Debt Ratio	0.068	0.105**	0.287
	(0.143)	(0.048)	(0.334)
Lagged Per Capita Expenditures	-0.042	0.006	0.046
	(0.038)	(0.012)	(0.113)
Lagged Log of Income Per Capita Interpolated	0.137***		
	(0.050)		
Lagged % Black Interpolated	-1.170**	0.457***	-0.514
	(0.533)	(0.090)	(0.839)
Lagged % Other Races Interpolated	0.211	0.182	-5.051**
	(0.737)	(0.294)	(2.085)
Lagged % > Age 60 Interpolated	-3.717***	0.372	-3.263
	(0.969)	(0.275)	(2.254)
Lagged % High School Degree Interpolated	-1.666***	0.088	-0.661
	(0.517)	(0.125)	(1.168)
Lagged % College Degree Interpolated	-2.059**	0.439**	5.212***
	(0.868)	(0.199)	(1.772)
Lagged % Manufacturing Jobs Interpolated	0.516	0.281**	-2.261*
	(0.583)	(0.123)	(1.176)
Lagged % Professional Jobs Interpolated	0.890	-0.057	-1.329
	(0.583)	(0.225)	(1.546)
Lagged % Vacant Units Interpolated	-0.532*		
	(0.304)		

Table 3.20 (cont'd)

	(1) Log Population	(2) Percent Unemployed	(3) Income Per Capita
Lagged % Owner Occupied Units Interpolated	0.489		
	(0.321)		
Observations	278	278	278
R-squared	0.99	0.669	0.991
Number of Cities	73	73	73

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of 73 of the 100 largest cities in the US (as of 2010) from 1970, 1980, 1990, 2000, and 2006.

Table 3.21: Mean Effect of Residency Requirement Laws on Population Using Yearly Census of Governments Estimates

	(1) Log Population
Lagged Residency Requirement Law	-0.031
	(0.024)
Lagged Log of Crime	0.157***
	(0.035)
Lagged Log of Property Tax Per Capita	-0.029
	(0.027)
Lagged Debt Ratio	0.194
	(0.174)
Lagged Per Capita Expenditures	-0.059
	(0.036)
Lagged Log of Income Per Capita Interpolated	0.702***
	(0.078)
Lagged % Black Interpolated	-1.118***
	(0.310)
Lagged % Other Races Interpolated	1.629***
	(0.591)
Lagged % > Age 60 Interpolated	0.098
	(0.882)
Lagged % High School Degree Interpolated	-1.371**
	(0.659)
Lagged % College Degree Interpolated	-2.263**
	(0.983)
Lagged % Manufacturing Jobs Interpolated	-0.348
	(0.384)
Lagged % Professional Jobs Interpolated	-1.676**
	(0.772)
Lagged % Vacant Units Interpolated	-0.804
	(0.755)
Lagged % Owner Occupied Units Interpolated	-0.015
	(0.211)
Observations	2,618
Number of Cities	73
R-squared	0.861

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of 73 of the 100 largest cities in the US (as of 2010) from 1970- 2006.

Table 3.22: Mean Effect of Residency Requirement Laws on Population Using Yearly Census of Governments Estimates with Time Trend

	(1) Log Population
Lagged Residency Requirement Law	-0.030
	(0.025)
Lagged Log of Crime	0.047**
	(0.019)
Lagged Log of Property Tax Per Capita	-0.010
	(0.015)
Lagged Debt Ratio	0.226
	(0.152)
Lagged Per Capita Expenditures	-0.008
	(0.024)
Lagged Log of Income Per Capita Interpolated	0.159***
	(0.047)
Lagged % Black Interpolated	-0.827***
	(0.312)
Lagged % Other Races Interpolated	-0.376
	(1.394)
Lagged % > Age 60 Interpolated	-4.384***
	(0.794)
Lagged % High School Degree Interpolated	-0.965*
	(0.506)
Lagged % College Degree Interpolated	0.134
	(1.226)
Lagged % Manufacturing Jobs Interpolated	0.543
	(0.791)
Lagged % Professional Jobs Interpolated	0.683
	(0.609)
Lagged % Vacant Units Interpolated	-0.397
	(0.398)
Lagged % Owner Occupied Units Interpolated	0.601**
	(0.255)
Observations	2,618
Number of Cities	73
R-squared	0.955

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of 73 of the 100 largest cities in the US (as of 2010) from 1970- 2006.

Table 3.23: Dynamic Effect of Residency Requirement Laws on Population Using Yearly Census of Governments Estimates

	(1) Log Population
Six years before Residency Law Implementation	0.019
	(0.015)
Five years before Residency Law Implementation	0.003
	(0.019)
Four years before Residency Law Implementation	0.011
	(0.019)
Three years before Residency Law Implementation	0.003
	(0.018)
Two years before Residency Law Implementation	0.006
	(0.022)
One year before Residency Law Implementation	0.005
	(0.024)
Year of Residency Law Implementation	-0.015
	(0.014)
One year after Residency Law Implementation	-0.010
	(0.009)
Two years after Residency Law Implementation	-0.007
	(0.010)
Three years after Residency Law Implementation	Omitted
Four years after Residency Law Implementation	0.004
	(0.009)
Five years after Residency Law Implementation	0.005
	(0.009)
Six or more years after Residency Law Implementation	0.019
	(0.014)
Lagged Log of Crime	0.098***
	(0.032)
Lagged Log of Property Tax Per Capita	-0.004
	(0.004)
Lagged Debt Ratio	0.182
	(0.167)
Lagged Per Capita Expenditures	0.045*
	(0.023)
Lagged Log of Income Per Capita Interpolated	0.571**
	(0.220)

Table 3.23 (cont'd)

	(1) Log Population
	(0.264)
Lagged % Other Races Interpolated	2.136
	(1.695)
Lagged % > Age 60 Interpolated	0.772
	(1.696)
Lagged % High School Degree Interpolated	0.507
	(0.732)
Lagged % College Degree Interpolated	-1.035
	(1.743)
Lagged % Manufacturing Jobs Interpolated	1.012**
	(0.376)
Lagged % Professional Jobs Interpolated	-0.837
	(1.089)
Lagged % Vacant Units Interpolated	-1.198*
	(0.597)
Lagged % Owner Occupied Units Interpolated	-0.766
	(0.590)
Observations	578
Number of Cities	34
R-squared	0.667

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of 73 of the 100 largest cities in the US (as of 2010) from 1970- 2006.

Table 3.24: Mean Effect of the Removal of Residency Requirement Laws on Population Using Yearly Census of Governments Estimates

	(1) Log Population
Lagged Removal of Residency Requirement Law	0.026
	(0.037)
Lagged Log of Crime	0.142***
	(0.040)
Lagged Log of Property Tax Per Capita	-0.029
	(0.028)
Lagged Debt Ratio	0.195
	(0.174)
Lagged Per Capita Expenditures	-0.070
	(0.042)
Lagged Log of Income Per Capita Interpolated	0.708***
	(0.081)
Lagged % Black Interpolated	-1.258***
	(0.353)
Lagged % Other Races Interpolated	1.501**
	(0.630)
Lagged % > Age 60 Interpolated	0.472
	(0.879)
Lagged % High School Degree Interpolated	-1.630**
	(0.754)
Lagged % College Degree Interpolated	-2.752**
	(1.134)
Lagged % Manufacturing Jobs Interpolated	-0.509
	(0.468)
Lagged % Professional Jobs Interpolated	-1.276
	(0.888)
Lagged % Vacant Units Interpolated	-0.763
	(0.770)
Lagged % Owner Occupied Units Interpolated	-0.106
	(0.226)
Observations	2,382
Number of Cities	73
R-squared	0.87

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of 73 of the largest cities in the US (as of 2010) from 1970- 2006.

Table 3.25: Mean Effect of the Removal of Residency Requirement Laws on Population Using Yearly Census of Governments Estimates with Time Trend

	(1) Log Population
Lagged Removal of Residency Requirement Law	0.054
	(0.036)
Lagged Log of Crime	0.049**
	(0.023)
Lagged Log of Property Tax Per Capita	-0.011
	(0.015)
Lagged Debt Ratio	0.248
	(0.156)
Lagged Per Capita Expenditures	-0.011
	(0.024)
Lagged Log of Income Per Capita Interpolated	0.178***
	(0.047)
Lagged % Black Interpolated	-1.146***
	(0.362)
Lagged % Other Races Interpolated	-0.440
	(1.380)
Lagged % > Age 60 Interpolated	-4.568***
	(0.907)
Lagged % High School Degree Interpolated	-0.878
	(0.579)
Lagged % College Degree Interpolated	0.124
	(1.183)
Lagged % Manufacturing Jobs Interpolated	0.418
	(0.787)
Lagged % Professional Jobs Interpolated	0.649
	(0.664)
Lagged % Vacant Units Interpolated	-0.426
	(0.418)
Lagged % Owner Occupied Units Interpolated	0.563*
	(0.305)
Observations	2,382
Number of Cities	73
R-squared	0.959

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of 73 of the largest cities in the US (as of 2010) from 1970- 2006.

Table 3.26: Dynamic Effect of the Removal of Residency Requirement Laws on Population Using Yearly Census of Governments Estimates

	(1) Log Population
Six years before Residency Law Removal	-0.011
	(0.023)
Five years before Residency Law Removal	0.001
	(0.026)
Four years before Residency Law Removal	-0.013
	(0.025)
Three years before Residency Law Removal	-0.013
	(0.025)
Two years before Residency Law Removal	-0.011
	(0.024)
One year before Residency Law Removal	-0.038
	(0.036)
Year of Residency Law Removal	-0.035
	(0.029)
One year after Residency Law Removal	-0.033
	(0.032)
Two years after Residency Law Removal	-0.022
	(0.036)
Three years after Residency Law Removal	-0.017
	(0.037)
Four years after Residency Law Removal	-0.015
	(0.040)
Five years after Residency Law Removal	-0.022
	(0.042)
Six or more years after Residency Law Removal	-0.021
	(0.043)
Lagged Log of Crime	0.145***
	(0.038)
Lagged Log of Property Tax Per Capita	-0.027
	(0.022)
Lagged Debt Ratio	0.042
	(0.122)
Lagged Per Capita Expenditures	0.008
	(0.028)

Table 3.26 (cont'd)

	(1) Log Population
	(0.076)
Lagged % Black Interpolated	-0.842**
	(0.370)
Lagged % Other Races Interpolated	1.472**
	(0.705)
Lagged % > Age 60 Interpolated	0.164
	(0.962)
Lagged % High School Degree Interpolated	-1.122
	(0.721)
Lagged % College Degree Interpolated	-2.698**
	(1.163)
Lagged % Manufacturing Jobs Interpolated	0.335
	(0.426)
Lagged % Professional Jobs Interpolated	-1.793*
	(0.966)
Lagged % Vacant Units Interpolated	-1.271
	(0.918)
Lagged % Owner Occupied Units Interpolated	0.117
	(0.232)
Observations	1,785
Number of Cities	72
R-squared	0.865

Robust standard errors in parentheses, clustered at the city level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is a panel of 73 of the largest cities in the US (as of 2010) from 1970- 2006.

BIBLIOGRAPHY

BIBLIOGRAPHY

Akiyama, Yoshio, and Sharon K. Propheter. (2005) "Methods of Data Quality Control: For Uniform Crime Reporting Programs." Criminal Justice Information Services Division. Federal Bureau of Investigation.

Angel, Cecil. "Detroiters tell legislators how ban on residency laws is hurting city." *Detroit Free Press* (Aug. 18, 2011). http://www.freep.com/article/20110819/NEWS01/108190413/Detroiters-tell-legislators-how-ban-residency-laws-hurting-city

Boies, Christopher G. "Fleming v. Giuliani." New York Law School Law Review. (2005/2006) 50:4 pp. 1003-1014.

City of Cincinnati. (1977). Ordinance of 29 June 1977.

Duncan, Brian. "Using Municipal Residency Requirements to Disguise Public Policy." *Public Finance Review*. 33: 84. (2005)

"Ector v. City of Torrance." (1973) 514 P 2nd 433.

Eisinger, Peter K. "Municipal Residency Requirements and the Local Economy." *Social Science Quarterly*. 64:1 (March 1983) p. 85

Fogelson, Robert. "Big City Police." Cambridge, Ma. Harvard University Press. (1977)

Glaeser, Edward L., Jose A. Scheinkman, Andrei Shleifer. (1995)"Economic Growth in a Cross-Section of Cities." National Bureau of Economic Research Working Paper No. 5013.

Gonzalez, Rodolfo A., Stephen L. Mehay, Kevin Duffy-Deno. "Municipal Residency Laws: Effects on Police Employment, Compensation, and Productivity." *Journal of Labor Research* (Fall 1991) 13:4

Hirsch, Werner Z., Anthony M. Rufolo. "Residence Laws and Unionization in Municipal Labor Markets: The Case of Firefighters." *Journal of Labor Research* (Winter, 1986) 7:1

International City Management Association. "Personnel Practices in the Municipal Police Service: 1976." *Urban Data Service Report* (Dec., 1976) pp. 2-14

Laasby, Gitte. "Firefighter group backs keeping residency rule." Journal Sentinel Online. (2011) http://www.jsonline.com/news/milwaukee/119739199.html

"Local hire laws: Alaska's futile attempts at preferential treatment." *Alaska Law Review* (Dec., 1987) pp. 359-386.

Lynch, James P. and John P. Jarvis. (2008). "Missing Data and Imputation in the Uniform Crime Reports and the Effects on National Estimats." *Journal of Contemporary Criminal Justice.* 24:69.

Maltz, M. (1999). "Bridging gaps in police crime data." Washington, DC: U.S. Bureau of Justice Statistics.

Mehay, Stephen L., Kenneth P. Seiden. "Municipal Residency Laws and Local Public Budgets." *Public Choice* (1986) 48:1 pp.27-35

Myers, Ross S. "The constitutionality of continuing residency requirements for local government employees: a second look." *California Western Law Review* (Fall 1986) 23 p 24-41

Nichols, Darren A. "Bank of America donating houses to entice Detroit cops to live in city." Detroit News (2011) http://detnews.com/article/20110323/METRO01/103230415/Bank-of-America-donating-houses-to-entice-Detroit-cops-to-live-in-city#ixzz1Jz84pw1v

New York News. (2011) "NYPD Facebook flap prompts call for residency rule." http://abclocal.go.com/wabc/story?section=news/local/new_york&id=8463511

O'Brien, Kevin M. "Do Municipal Residency Laws Affect Labour Market Outcomes?" *Urban Studies* (1997) 34:11 pp. 1759-1769

Plerhoples, Christina. (2013) "Motivations for the Implementation of Municipal Residency Requirement Laws." PhD Dissertation.

Reed, Thomas. "Municipal Management." New York: McGraw Hill. (1941)

Sachs, Jeffrey D. (2003) "Institutions Don't Rule: Direct Effects of Geography on per Capita Income." National Bureau of Economic Research Working Paper 9490.

Smith, Dennis C. "Police Attitudes and Performance: The Impact of Residency." *Urban Affairs Review* (1980) 15:317

Stein, Jason, and Don Walker. (2013). "Local officials, some GOP lawmakers sound off against residency change." http://www.jsonline.com/news/statepolitics/local-officials-some-gop-lawmakers-sound-off-against-residency-change-179gnlk-202418121.html

Stein, Lana. "Representative Local Government: Minorities in the Municipal Work Force." *The Journal of Politics* (Aug., 1986) 48:3 pp. 694-713

Wooldridge, Jeffrey. 2002. Econometric Analysis of Cross Section and Panel Data. Cambridge, MA: MIT Press.